The Nineteenth
Century Series

THE NINETEENTH CENTURY SERIES

PROGRESS OF SCIENCE

IN THE CENTURY

BY

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PREFACE.

To discuss in a single volume the progress of science in the nineteenth century has been no easy task, and the author craves the reader's indulgence. It must be remembered that the book does not pretend to be a history of nineteenth century science; it is designed simply as an introduction to many histories—some still unwritten. It is not a consecutive story of the marvellous progress of knowledge which the century witnessed; it is simply a record of some of the great scientific events. Many famous names and many important discoveries have been left unmentioned, for any attempt at exhaustiveness would have made a volume of this size a mere catalogue. On the other hand, there has been a serious attempt to discuss the great theme so as to give prominence to the salient steps of progress. To have attempted this in an easy-going mood would have been irreverent to the past and insulting to the serious reader; therefore no apology is offered for the difficulty of some of the pages, nor does it seem necessary to apologise for the numerous quotations from expert authorities,—they help to give personal reality to some of the pages, and they were needed as acknowledgments of the author's indebtedness. J. A. T.

University of Aberdeen, September, 1902.

Note.—The reader will understand that the absence of any reference to radium and its marvellous properties is due to the fact that the book was printed before the discovery had been made. In the same way it will be obvious why Sir Oliver Lodge and Sir William Ramsay are not duly entitled, and why some great men of science no longer with us, such as Gegenbaur, Spencer, and Zittel, are referred to in the present tense.

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PROGRESS OF SCIENCE IN THE CENTURY.

BOOK ONE.

INTRODUCTORY.

CHAPTER I.

The Scientific Mood.

THE MEANING OF SCIENCE.

Many attempts have been made to define what we mean by "Science." "A higher development of common knowledge" (Spencer); "organised common sense" (Huxley); "classified and criticised knowledge"; "the universal element in knowledge"; "an understanding of facts"; "our correlated experience,"—are among the many suggestions. It will be noted that these definitions, though all somewhat vague, suggest two ideas: (*a*) that science is not something by itself, apart from other knowledge, or confined to any particular order of facts; and (*b*) that it has none the less a distinctive feature, as expressed by some word like "organised" or "systematised." The fact is that whenever we gather

facts and classify them, detect their inter-relations and formulate their sequences, there is science. The subject of enquiry may be man or beast, star or tree, a language or the atmosphere, institutions or fossils, the growth of ideas or the development of an egg—all come within the scope of scientific enquiry whose far-off goal is an interpretation of the known world. The distinctive feature is in the method,—making sure of facts, observing their inter-relations, grouping them according to their likenesses of sequence, and inventing descriptive formulæ which sum them up. Facts are essential, but it is evident that they alone do not constitute a science; they must be correlated, interpreted, formulated. As Sir Lyon Playfair once put it,* "isolated facts may be viewed as the dust of science," —dust only, but dust is not to be despised, for, as he went on to say, "to it when the rays of light act upon its floating particles we owe the blue of the heavens and the glories of the sky."

Though it may sound for a moment like a paradox, the scientific mood does not necessarily involve any particular knowledge of this or that science. Many business men, for instance, who are almost quite ignorant of chemistry or physics, botany or zoology, astronomy or geology, but who have carefully disciplined themselves in regard to some restricted series of facts involved in their daily work, have acquired the scientific mood in a high degree of development. The same may be said of many a one well disciplined in the "Humanities," though his title of "scholar" is often used as if it stood in antithesis to "man of science."

* Pres. Address, *Rep. Brit. Ass.* for 1885, p. 18.

A CONTRAST OF MOODS.

We receive in our inheritance what may be metaphorically called a bundle of moods—of various shapes and sizes, like a bundle of sticks gathered in the forest. Among these moods, or predispositions to particular lines of activity, three stand out prominently—the scientific, the artistic, and the practical mood. Most of us have at least the rudiments of these, but in most cases one is dominant. It is part of the aim of education to adjust the proportions of our moods, and to foster a minute rudiment into realisation. First there is the mood of the dominantly *practical* man, who, though in part scientific and usually a man of feeling, is characteristically concerned with the possibilities of *action*. The whole trend of his mind is towards doing, not towards knowing. He is seeking after social amelioration, not after descriptive formulæ.

There is obviously much to be said for the dominance of the practical mood. It seems likely that man's first relations to nature were predominantly practical, and it is certain that in old practical lore many of the sciences—such as astronomy, botany, physiology—had their roots, and that fresh vigour has often come to science by a tightening of its contact with the affairs of daily life. There is no doubt that the practical mood is as natural and necessary and dignified as any other. Without it science tends to become pedantic and art decadent. Yet when the practical mood becomes altogether dominant, when things get into the saddle and over-ride ideas and ideals and all good feeling, when the multiplication of loaves and fishes becomes the only problem in the world, we know the results to be vicious. The

vices of the hypertrophied practical mood are—belittlement, baseness, brutality. We cannot but have a great respect for the dominant practical mood, and yet if it is left unchecked by scientific discipline and artistic culture, it tends to run riot. The practical man elects to do, not know, but action without knowledge is often our undoing. Ignorant practice may be more dangerous than any dogma. The practical man will have "nothing to do with sentiment," though he prides himself in keeping close to the facts; he cannot abide any theory and yet he is imbued with a Martin Tupperism which gives a false simplicity to the problems of life; he will live in what he calls "the *real* world," and yet he often hugs close to himself the most unreal of ideals.

Secondly, there is a man of dominantly artistic mood, which seems to find expression in Schiller's words:—"*O wunderschön ist Gottes Erde, und schön auf ihr ein Mensch zu sein;*" "How beautiful is God's earth, how good it is to live a man's life upon it."

From man's first emergence until to-day, the drama of nature has doubtless appealed to human emotions. Especially, perhaps, as he gained firmer foothold in the world, secured by his wits against stronger rivals and a careless environment, did the emotional tone rise into dignity as a distinct mood, finding its expression in painting and carving, song and story, music and the dance. The herbs and the trees, the birds and the beasts, sent tendrils into the human heart, claiming and finding kinship.

Like the practical mood, so the emotional mood has its obvious virtues. It is part of the salt of life. In a noisy world it helps to keep us aware of the harmony in the heart of things.

Yet it has its vices; if unruled or uncorrelated, if uncurbed by science, if unrelated to the practical problems of life, it tends to become morbid, mawkish, mad. There may be over-feeling, just as there may be over-doing. Most serious consequences of feeling without knowledge, of sympathy without synthesis (in the language of the learned), are well known in the practical affairs of to-day.

On the other hand, we must not be slow to admit that just as the practical man has some justification when he reacts from science, because, as he says, it is too theoretical, so the artist, poet, or man of feeling has some justification when he recoils from science because it is disproportionately analytic. It must be granted that science, like a child pulling a flower to bits, is apt to dissect more than it reconstructs, and to lose in its analysis the vision of unity and harmony which the artist has ever before his eyes. Perhaps, however, if the artist had patience, he would often find that science restores the unity with more meaning in it than before.

Thirdly, there is the dominant scientific mood. To this mood the world-picture is no phantasmagoria, but a scene in an ordered drama; even its beauty is not kaleidoscopic but rather of growth. To the scientific mood it is plain that through the multiplicity of items great likenesses are observable, which admit of being summed up in brief descriptive formulæ—laws of motion, gravitation, indestructibility of matter, conservation of energy, development from the apparently simple to the obviously complex evolution.

Although science has some of its roots in practice, and often receives stimulus from the actual needs of the day, it is not practical either in main inten-

tion or in main result. Its main intention is to describe in the simplest possible formulæ, to classify and inter-relate sense-impressions, to interpret the known world; its main result is an intellectual system and the development of a certain way of looking at things.

Similarly, though emotion has influenced the growth of natural knowledge not a little both for good and ill, and though scientific discoveries have in turn given nutriment to emotion, science is certainly in itself non-emotional.

The student of science seeks, not like the practical man, to realise the ideal, but rather to idealise [conceptualise] the real, or those fractions of reality which constitute his experience. He elects primarily to know, not do. He would make the world translucent, not that emotion may catch the glimmer of the indefinable light that shines through, but for other reasons,—because of his inborn inquisitiveness, because of his dislike of obscurities, because of his craving for a system—an intellectual system in which phenomena are provisionally unified.

Like the other moods, the scientific mood has its virtues of method and ideal. It is painstaking, patient, precise; it is careful, conscientious, contriving; it aims at making a working thought-model of the universe.

But it has also its vices,—of over-knowing, of ranking science first and life second (as if science were not after all *for* the evolution of life), of ignoring good feeling (as if knowledge could not be bought at too dear a price), of pedantry (as if science were a "preserve" for expert intellectual sportsmen, and not an education for the citizen), of maniacal muck-raking for items of facts (as if facts alone consti-

tuted science). But it is a natural and necessary expression of the developing human spirit, and supplies the foundation without which practice is merely empirical and emotion superstitious.

CHARACTERISTICS OF THE SCIENTIFIC MOOD.

In his stimulating presidential address at the meeting of the British Association at Dover in 1899, Sir Michael Foster raised the question of the distinctive features of the scientific spirit. "What are the qualities," he asked, "the features of that scientific mind which has wrought, and is working, such great changes in man's relation to nature?" And his answer was that the features of the fruitful scientific mind are in the main three.*

"In the first place, above all other things, his nature must be one which vibrates in unison with that of which he is in search; the seeker after truth must himself be truthful, truthful with the truthfulness of nature; which is far more imperious, far more exacting than that which man sometimes calls truthfulness.

"In the second place, he must be alert of mind. Nature is ever making signs to us, she is ever whispering to us the beginnings of her secrets; the scientific man must be ever on the watch, ready at once to lay hold of Nature's hint, however small, to listen to her whisper, however low.

"In the third place, scientific enquiry, though it be pre-eminently an intellectual effort, has need of the moral quality of courage—not so much the courage which helps a man to face a sudden difficulty as the courage of steadfast endurance."

* *Report British Association for the Advancement of Science*, 1899, pp. 16–17.

To the obvious objection that these three qualities of truthfulness, alertness, and courage, though, let us hope, possessed by the scientific man, are not in any way peculiar to him, but "may be recognised as belonging to almost every one who has commanded or deserved success, whatever may have been his walk in life," Sir Michael answered: "That is exactly what I would desire to insist, that the men of science have no peculiar virtues, no special powers. They are ordinary men, their characters are common, even commonplace. Science, as Huxley said, is organised common sense, and men of science are common men, drilled in the ways of common sense."

Let us endeavour to make the diagnosis of the scientific mood a little more definite. The following has at least the interest of having been almost entirely written before the delivery of Sir Michael Foster's stimulating address.

(*a*) As a first characteristic of the scientific mood—corresponding to what has been above referred to as "truthfulness," we may note *a passion for facts*. And what are more difficult to catch than facts; they are more elusive than ideas. How difficult it is even in regard to simple problems to get a grip of the facts of the case! How difficult it is for any one with even a dash of the artistic mood to relate an occurrence accurately! Most of us are Munchausens in a small way, but with less sense of humour. Just as we may distinguish carpenters who can work to this or that fraction of an inch of accuracy; so we must distinguish one another as able to observe or to record to this or that degree of truthfulness.

"Man, unscientific man, is often content with 'the nearly' and 'the almost.' Nature never is. It is not her way to call the same two things which

differ, though the difference may be measured by less than the thousandth of a milligramme or of a millimetre, or by any other like standard of minuteness. And the man who, carrying the ways of the world into the domain of science, thinks that he may treat Nature's differences in any other way than she treats them herself, will find that she resents his conduct; if he in carelessness or in disdain overlooks the minute difference which she holds out to him as a signal to guide him in his search, the projecting tip, as it were, of some buried treasure, he is bound to go astray, and, the more strenuously he struggles on, the farther will he find himself from his true goal."*

Many people—most excellent in virtues—seem constitutionally incapable of accurately reporting an occurrence; many more seem quite unable to see the difference between an observation and an inference.

The scientific worker is himself well aware that, in measurements and observations, only an approximate accuracy can be attained, and that the degree of approximation varies with the individual. But this relativity of accuracy is far from being generally recognised, and scientific statements often get credit for a precision which they do not claim. The personal equation has been for a long time frankly recognised and allowed for in astronomy; it is also sometimes estimated in chemistry and physics,† but we hear too little of it in the less exact sciences such as biology and psychology.

Even apart from intellectual training, may it not be claimed that the discipline of the chemical balance,

* Sir Michael Foster, *loc. cit.* p. 16.

† See Ostwald's *Text-book of General Chemistry.*

of analysis, of dissection, of faithful drawing, is one of the most effective factors in the evolution of truthfulness? Many will agree with Agassiz that some training in natural science is one of the best preparations a man can have for work in any department of life where accurate carefulness and adherence to the facts of the case means much. Long ago Bacon said: "We should accustom ourselves to things themselves," and this—to distinguish between appearance and reality—is what the scientific mood seeks after.

It was Huxley who spoke of "that enthusiasm for truth, that fanaticism of veracity, which is a greater possession than much learning; a nobler gift than the power of increasing knowledge." It is one of the motive forces of scientific progress.

If every virtue has its vice and every function its disease, so danger may lurk in this precious possession,—a passion for facts. It may become a mania for information and an intellectual intemperance. Unskilful teaching or careless learning may result in mere fat without muscle, or in the matter-of-fact man—one of the most unscientific of persons—who ignores one of the biggest of all facts, the reality of ideas.

Any mood may in extreme development become vicious, and the passion for facts may become so predominant that it implies violence to emotional sanity and disloyalty to the ideal of a full and healthy human life. Take an illustration from real life. The great embryologist Von Baer once shut himself up in his study when snow was upon the ground, and did not come out again until the rye was in harvest. He was filled, he tells us, with uncontrollable pathos at the sight. "The laws of development may be

discovered this year or many years hence—by me or by others—what matters it?—it is surely folly to sacrifice for this the joy of life which nothing can replace." Indeed life is not for science, but science for the development of life.

These are days of popularising, in magazine articles and on lecture platforms, and much of this is justifiable and healthy, for science can no longer be defined off as a preserve for the learned. Yet there is the risk of giving a false simplicity to problems, or of suggesting that there are royal roads to learning; the sin easily besets us of depreciating the dignity of a hard-won fact. Therefore at the risk of exceeding triteness, we would emphasise that a genuine passion for facts implies a certain seriousness, a reverence for what is beneath (in Goethe's words), a respect for facts when one gets them. Though we need not be always in the scientific mood—for which we are truly thankful—we must be scientific when we propose so to be. "Science," Bacon said, "is not a terrace for a wandering and variable mind to walk up and down with a fair prospect."

What we mean by saying that we need not be always scientific is simply that the scientific mood is sometimes unnatural and irrelevant. To botanise upon our mother's grave is the classic illustration, and for another we may refer to the medical man's discovery that Botticelli's "Venus," in the Uffizi at Florence, is suffering from consumption, and should not be riding across the sea in an open shell, clad so scantily.

(*b*) Following from the passion of facts, is a second characteristic of the scientific mood, namely, cautiousness, or distrust of finality and dogmatism of statement. Scotsmen have done well for the ad-

vancement of science; they are said to stand far above the average in the nineteenth century; perhaps this is in part because they are so "canny," so unwilling to commit themselves unless they are sure. It may even be that the excessive changeableness of Scotch weather has helped to engender the characteristic mood of caution. Sometimes, indeed, the cautiousness becomes almost morbid, when three saving clauses are inserted in a single sentence. One recalls Stevenson's story of the sailor:—"Bill, Bill," says I, "or *words to that effect.*"

No doubt the scientific mood is continually making hypotheses or guesses at truth; the scientific use of the imagination is part of our method. But what we have to guard against is the insidious tendency to mistake provisional hypotheses for full-grown theories, and, still worse, for dogmas.

As Prof. W. K. Brooks says in his *Foundations of Zoology*: "The hardest of intellectual virtues is philosophic doubt, and the mental vice to which we are most prone is our tendency to believe that lack of evidence for an opinion is a reason for believing something else. . . . Suspended judgment is the greatest triumph of intellectual discipline." As Huxley said —and who has had the scientific mood more strongly developed—"The assertion that outstrips the evidence is not only a blunder but a crime." Just as burnt bairns dread the fire, so the scientific mood, often deceived by hearsay evidence, by incomplete induction, by the will-o'-the-wisp glamour of a seductive idea, by inference mixed up with observation, and even by wilful falsehood, becomes more and more cautious, distrustful, "canny."

Another aspect of the quality of cautiousness which characterises the scientific mood is distrust of

personal bias in forming judgments. It should always be possible to eliminate *opinion* from all scientific conclusions; their validity, in fact, depends upon this. "The scientific man has above all things to strive at self-elimination in his judgments, to provide an argument which is as true for each individual mind as for his own. The classification of facts, the recognition of their sequence and relative significance, is the function of science, and the habit of forming a judgment upon these facts, unbiassed by personal feeling, is characteristic of what may be termed the scientific frame of mind." *

"The world," Faraday writes, "little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realised." As a complementary statement, another quotation from the same great authority may be permitted:—"The philosopher should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by appearances; have no favourable hypotheses; be of no school, and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the Temple of Nature."

(*c*) A third characteristic of the scientific mood is dislike of obscurities, of blurred vision, of fogginess. We instinctively discount the scientific abili-

* Karl Pearson, *Grammar of Science*, rev. edition, 1900, p. 6.

ties of the student who always has his microscope wrongly focussed and is satisfied with the ill-defined image, or of the other whose dissection is invariably either a mince or a tangle, or of the other who is never quite sure whether he knows a thing or not. Ignorance in itself is no particular reproach; the point is to know when we know and when we don't, and it is one of the characteristics of the scientific mood that it will have yes or no to this question.

Those of the scientific mood are mainly trying to construct a working-thought-model of the outer world, to form a mental image which will be a living picture,—an intellectual cinematograph. In other words they would make the world translucent, as translucent as the human body becomes to the skilled anatomist.

Clerk-Maxwell's boyish question—"What is the go of this?"—and, when put off with some verbalism, "What is the *particular* go of this?" is a question characteristic of the scientific mood, which may be applied to any order of facts.

The mole has a sort of half-finished lens, which is physically incapable of throwing a precise image on the retina. If there is any image, it must be a blurred tangle of lines. In our busy lives, we tend to acquire mole-like lenses in regard to particular orders of facts; we see certain things clearly, others are blurs; but the scientific mood is in continual protest against obscurities, insisting upon lucidity.

Thus we feel the force of one of Bacon's most historically true aphorisms, which declares "Truth to emerge sooner from error than from confusion." It is a great step when a false notion is formulated. The definitising of error has been the beginning of its disappearance. As soon as the evil genie of the

Eastern tales took on some definite bodily form there was some chance of tackling him; as a mere wraith he was invulnerable.

(*d*) A fourth characteristic of the scientific mood is a sense of the inter-relations of things. The realisation of nature as a great inter-connected system is, indeed, one of the ends of science; to be on the outlook for inter-relations is diagnostic of the mood. As long as the collection and registration of facts preoccupies the energies and attention, scientific enquiry has hardly begun. As Mr. Pearson says, "The classification of facts, the recognition of their sequence and relative significance is the function of science."

To put it more concretely, the student of biology, for instance, has hardly caught on at all unless he has some realisation of the web of life, the correlation of organisms. He must have some appreciation of the "system of nature," of the links between old maids, cats, bees, and clover crop; between earthworms and the world's bread-supply; between mosquitoes and malaria; between white ants and African agriculture; between ivory ornaments and the slave trade.

To sum up: the scientific mood, whose diffusion through wide circles has been one of the achievements of the latter half of the nineteenth century, is characterised by a passion for facts, an alert cautiousness, a striving after clearness of vision, and a sense of inter-relations. To which, as will be afterwards made plain, it should perhaps be added that the consistent scientific mood does not at all concern itself with metaphysical problems or ultimate interpretations. These may be legitimately complementary to science, but if the word is to retain its present meaning, they are beyond its scope.

THE AIM OF SCIENCE.

Briefly stated, the primary aim of science includes the observation, description, and interpretation of the knowable universe.

Concerning the need for careful observation and accurate description, enough has been said in our exposition of the characteristics of the scientific mood; it is necessary, however, to give particular attention to the nature of a scientific interpretation,—in regard to which misunderstanding is rife.

The man of scientific mood becomes aware of certain fractions of reality which interest him; he proceeds to become more intimately aware of these; i.e., to make his sensory experience of them as full as possible. He seeks to arrange them in ordered series, to detect their inter-relations and likenesses of sequence; he tries to reduce them to simpler terms or to find their common denominator; and finally, he endeavours to sum them up in a general formula, often called a "law of nature."

Aristotle defines the aim when he says, "Art [or as we should say, Science] begins when, from a great number of experiences, one general conception is formed which will embrace all similar cases." Similarly the nature of scientific explanation is suggested by Kirchhoff's definition of mechanics, as the science of motion, whose object it is "to describe completely and in the simplest manner the motions that occur in nature."

With the advance of clear thinking our way of looking at facts has altered not a little, and even when we use the same words as our forefathers did we do not always mean the same thing. Thus when

the lecturer says that a gas "obeys Boyle's Law," he is using the language of the past and suggesting a conception of the order of nature which is no longer current. "We must confess," says Prof. J. J. Poynting,* "that physical laws have greatly fallen off in dignity. No long time ago they were quite commonly described as the Fixed Laws of Nature, and were supposed sufficient in themselves to govern the universe. Now we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed."

Prof. Poynting goes on to say that a "law of nature explains nothing—it has no governing power, it is but a descriptive formula which the careless have sometimes personified. There may be psychological and social generalisations which really tell us *why* this or that occurs, but chemical and physical generalisations are wholly concerned with the *how*."

In other words, if we may condense a little of Poynting's admirable discourse, concurrently with the change in our conception of physical law has come a change in our conception of physical explanation. The change is in our recognition that "we explain an event not when we know 'why' it happened, but when we know 'how' it is like something else happening elsewhere or otherwise—when, in fact, we can conclude it as a case described by some law already set forth. In explanation we do not account *for* the event, but we improve our account *of* it by likening it to what we already knew." In short, the notion of antecedent purpose—which rises

* Address, Section A, *Report of British Ass.* for 1889, pp. 616–17.

at once in our minds when we try to explain human action—is irrelevant in physical science.

On the same subject, Dr. J. T. Merz writes as follows in his impressive history of scientific thought in the nineteenth century: "A complete and simple description—admitting of calculation—is the aim of all exact science. . . . We shall not expect to find the ultimate and final causes, and science will not teach us to understand nature and life. . . . Science means 'the analysis of phenomena as to their appearance in space and their sequence in time.'" *

Thus the common assertion that science gives explanations of nature is a misunderstanding, if the word explanation is taken to mean more than a descriptive formula. The word ultimate does not occur in the scientific dictionary. The biologist draws cheques, but they are all backed by such words as protoplasm and germ-plasm; and a little enquiry suffices to show that these words imply conceptual hypotheses invented to express the facts and warranted by the success with which they fit these. The physicist's bills, similarly, are accepted on the credit of the ubiquitous ether, the mighty atom, or the like, but these again are conceptual hypotheses invented to summarise the sequence of phenomena.

Let us take a concrete case. "The law of gravitation is a brief description of *how* every particle of matter in the universe is altering its motion with reference to every other particle. It does not tell us *why* particles thus move; it does not tell us *why* the earth describes a certain curve round the sun. It simply resumes, in a few brief words, the relation-

* J. T. Merz. *A History of European Thought in the Nineteenth Century*. Vol. I., Introduction—Scientific Thought, Part I., 1896, pp. 382–3.

ships observed between a vast range of phenomena. It economises thought by stating in conceptual short-hand that routine of our perceptions which forms for us the universe of gravitating matter." *

SCIENTIFIC METHOD.

From what we have already said it should be plain that science has no mysterious methods of its own. Its method is the method of common sense. In his little book on scientific thinking,† Dr. Adolf Wagner points out with great vivacity that science is characterised as an intellectual attitude; it is not any particular body of facts; it has no peculiar method of inquiry; it is simply sincere critical thought, which admits conclusions only when these are based on evidence. Let us, however, briefly indicate some of the chief steps in the scientific treatment of a given problem.

(*a*) *Observation of Facts.*—The first step is to make sure of the facts concerning which a problem has been raised in the inquisitive mind. Here the fundamental virtues are precision, caution, clearness, and impartiality. The rough and ready record, the second-hand evidence, the vague impression, the picking of facts which suit must be eliminated. Hence, since the observer is a fallible mortal, the importance of co-operation, of independent observation on the same subject, of instrumental means of extending the range and delicacy of our senses, and of automatic methods of registration, such as photography supplies.

* Karl Pearson, *The Grammar of Science*, rev. ed., 1900, p. 99.

† A. Wagner, *Studien und Skizzen aus Naturwissenschaft und Philosophie. I. Ueber wissenschaftliches Denken und über populäre Wissenschaft*, Berlin, 1899, p. 79.

(*b*) *Classification of Facts.*—In many cases after the accumulation of data, much time must be spent in their arrangement. A careful worker at the problem of migration in birds, like Mr. Eagle Clarke, may require for the classification of his data a longer time than was spent in their collection. If the facts are to form part of the body of science, they must be made readily available, and this process of digestion is often slower than that of ingestion. If the aim be to detect similarities of sequence the facts must be grouped in ordered series. Here, in many cases, the use of graphs, curves, and mathematical methods has proved itself invaluable, notably, for instance, in Galton's work on inheritance, or in the recent statistical studies on variation.

It has been a common experience in the arrangement of data that some minute discrepancy has revealed itself, and that the following of this at first perhaps puzzling occurrence has led to the elucidation of the whole problem. Thus it has become a maxim in science that no apparent departure from the rule or general sequence should be treated as trivial, and no minute discrepancy disregarded. That nitrogen obtained from chemical combinations should be about one-half per cent. lighter than that obtained from the atmosphere, may seem a very minute fact, but it led Lord Rayleigh and Professor W. Ramsay to the discovery of Argon.

(*c*) *Analysis.*—With scientific problems of a certain order, there is often need for a preliminary process of analysis before the desired data can be obtained. Whenever we get below the surface phenomena of life—patent to the observer—we have to dissect, to cut sections, to take advantage of chemical analysis and so on. The end desired is a re-state-

ment in simpler terms, or in another sense, in more generalised terms; and to effect this analysis is in itself a scientific problem.

(*d*) *Hypothesis.*—There is no doubt that some conclusions have arisen in the mind as if by a flash of insight, but even these have perhaps been due to processes of unconscious cerebration. In the majority of cases, the process is a slower one, the scientific imagination devises a possible solution—an hypothesis—and the investigator proceeds to test it. In other words, he forges intellectual keys and then tries if they fit the lock. If the hypothesis does not fit, it is rejected and another is made. The scientific workshop is full of discarded keys. Nor can it be forgotten that even those conclusions which commend themselves at first sight have to submit to the process of testing like those which were tried with less confident fingers. It matters little, except to the logician, whether the hypothesis was reached as an induction from many particulars or as a deduction from some previously established conclusion; in either case the result is a provisional hypothesis, which has then to be tested.

Newton said in his *Principia* that he did not make hypotheses (*Hypotheses non fingo*), and yet he, like all great scientific workers, certainly did, for instance in his corpuscular hypothesis of light, which has turned out to be erroneous. The fact is that there are different kinds of hypotheses,—there are guesses at truth which have no experimental basis, which are usually prompted by some big conclusion dominating the mind of the guesser, such as Swedenborg's nebular hypothesis; and there are scientific hypotheses which are more or less carefully constructed systems, harmonised with existing knowl-

edge, and projected upon nature to satisfy our desire for continuity. They relate to what lies beyond the range of observation, beyond the range of our sense-impressions.

An interesting method of testing the accuracy of a formula is to use it as a basis for prediction. Many observant people are familiar with a mild form of scientific prophecy in connection with the weather. After long observation they hazard a generalisation, in private, if they are wise; and they test this by a prediction. As this is usually wrong, they conclude that their generalisation had not a sufficiently wide basis. But better examples may be found in the prediction of Neptune by Adams and Leverrier (from calculations based on the gravitation-formula) and the subsequent discovery of that planet by Galle; or in the prediction of the element germanium by Mendelejeff and its discovery by Winkler.

(*e*) *Test Experiments and Control Experiments.*—The distinction between an observation and an experiment seems quite artificial, the point of contrast being that in the former we study the natural course of events, while in the latter we arrange for the occurrence of certain phenomena. In studying the effect of electric discharges on living plants we might wait for the lightning to strike trees in our vicinity; but as this would be worse than tedious, we prefer to mimic the natural phenomenon in the laboratory. This is obviously a distinction without a difference, and instead of calling the first step (*a*) observation, as we have done, we might equally well have used the word experiment.

On the other hand, at a later stage in the scientific treatment of a problem, our opportunities for experiment can be profitably used, not for accumulating

more data, but for putting our hypothesis to the proof. We allude to what are called test or crucial experiments and control experiments. Much of the success of a scientific worker may depend on his ingenuity in thinking out crucial experiments and on his rigorous use of control experiments.

When bacteriology was in its infancy, Pasteur put his theory that putrefaction was the result of the life of micro-organisms to a crucial test when he sterilised readily putrescible substances, and, having hermetically sealed the vessel, kept them for years without the occurrence of any putrefaction.

When Von Siebold and his fellow-workers had gradually convinced themselves that certain bladderworms in various animals used as food were the young stages of certain tapeworms occurring in man, they made the crucial experiment of swallowing the bladderworms and proved the accuracy of their conclusion by becoming shortly afterwards infected with tapeworm.

The control experiment is closely akin. A crayfish is known to have a sense of smell. Various reasons lead the enquirer to conclude that this sense has its seat in the antennules. He may verify this by observing that a crayfish without these appendages will not respond to a strong odour, but he would not be satisfied unless he had shown that in exactly the same conditions and to exactly the same stimulus another crayfish with its antennules intact did actively respond. Having gone so far, he would proceed to localise the sense more precisely; microscopic research would direct his attention to peculiarly shaped bristles on the antennules. By shaving these off, and observing that response to strong odours ceased, he would prove his point, but again, in view of possible

error, he would confirm his conclusion by control experiments with normal animals. The above case illustrates a combination of the method of exclusion with the use of control experiments.

(*f*) *Formulation and Incorporation.*—The final step is to sum up what has been attained in terms as clear and terse as possible, and to add the discovery to what has been already established. The digested data are absorbed into the body of science. If the discovery is one of magnitude it will be expressible as a formula, which should have the criterion of universal validity in the minds of all who are able to estimate the evidence. But even here, in our judgment, there should arise the final question of considering how the new generalisation consists with others, or in wider terms, how it is related to the sum of human experience. Should it be markedly inconsistent, as the evolution-formula seemed at first to so many, there may be need for re-consideration. The body may have to adapt itself—possibly not without pain—to its new food.

Finally, to quote once more from Prof. Karl Pearson: "The scientific method is marked by the following features:—(*a*) careful and accurate classification of facts and observation of their correlation and sequence; (*b*) the discovery of scientific laws by aid of the creative imagination; and (*c*) self-criticism and the final touchstone of equal validity for all normally constituted minds."

CHAPTER II.

The Unity of Science.

CLASSIFICATION OF THE SCIENCES.

Since science presumes to take the whole universe for its province, and faces the immense problem of the order of nature, it is not surprising that a division of intellectual labour has been found convenient, and that separate sciences have been defined off, each with particular problems and special methods. This is an adaptation to the shortness of human life and the limitations of human faculty, for while there is nothing but laziness and mis-education to hinder an intelligent citizen from having scientific interest in all orders of facts, the long discipline which a science requires renders it impossible that any average man will succeed in gaining masterly familiarity with more than one department of knowledge.

The title of the old Scotch professorships of "Civil and Natural History" perhaps expressed more than one good idea,—for instance, that man must be studied in relation to his environment, or, again, that the history of non-human organisms might have some light to throw upon the history of mankind, but the ideal suggested was too ambitious for ordinary mortals. The fact is that a compromise has to be made between two desirabilities. On the one hand, the

aim of science-teaching, which is a culture of the scientific mood and an appreciation of scientific method, seems more likely to be attained by a thorough study of some one order of facts than by an intellectual ramble through the universe; on the other hand, the true dignity and value of science cannot be appreciated if the unity of nature and of knowledge be practically denied. Superficiality results from lack of specialisation, and pedantry from too much of it. Let us briefly consider some of the classifications which have been found convenient.

Francis Bacon (1561–1626) recognised three departments of human learning: (1) History (based on memory) both "natural" and "civil"; (2) Poesy (based on imagination); and (3) Philosophy or the Sciences (based on reason), including Divinity, which has to do with revelation, and Natural Philosophy, which deals with God, Nature, and Man! There is little in this classification which can be of service to us to-day in mapping out the territory of science, but it is interesting (as Karl Pearson points out) to notice the suggestion that "The divisions of knowledge are not like several lines that meet in one angle, but are rather like branches of a tree that meet in one stem." Auguste Comte (1798–1857) recognised six fundamental sciences: Mathematics, Astronomy, Physics, Chemistry, Biology, Sociology—and a supreme or final science of Morals. He sought to eliminate from his system all that is not based on experience, and he introduced the important conception of a hierarchy of knowledge, that is to say the idea that one department of science is dependent on another, sociology on biology, biology on chemistry, chemistry on physics, and so on. Without pretending that

the facts of life can be re-stated in terms of chemistry and physics, or that the biologist has given into his hands the key to the problems of human society, we may profitably recognise that an understanding of the organism is facilitated by the results of chemical and physical science, and that the data of biology are full of suggestion to the sociologist.

It may be true—many would call it obvious—that life transcends the categories of mechanism, or, in other words, that the formulæ of physics do not suffice to re-express the facts of life. Yet it must be admitted that vital phenomena have become more intelligible—more readily dealt with in thinking—since Biology began to avail itself of the aid of Chemistry and Physics. It may be true that man transcends the categories of Biology, and it seems to many that man as compared with the Amœba expresses *an entirely new synthesis,* just as the Amœba does in relation to a mineral, and that the secret of both new syntheses remains as yet hidden. Yet it must be admitted that human life has become more intelligible—more readily dealt with in thinking—since Psychology and Sociology condescended to listen to the suggestions, confessedly still immature, offered by Biology. On the other hand, it seems historically true that such valuable ideas as division of labour and evolution were made clear in regard to human affairs before they were transferred to and re-illustrated in the study of organisms. There is a sense in which the Amœba may be said to be of use in the interpretation of man; but it is also true that the study of man has reacted upon the biological interpretation of the Amœba. Similarly great advances were made by Chemistry when attention was extended from inorganic to organic substances, and there are at least

hints that the application of the Evolution-idea to the problems of the inorganic will make for progress. It was this idea of the interdependence of different scientific disciplines which especially marked Comte's classification. Herbert Spencer (1864) "combined the 'tree' system of Bacon with Comte's exclusion of theology and metaphysics from the field of knowledge," * and he focussed the distinction between the *Abstract* sciences of Logic and Mathematics (which deal with our methods of conceptual description) and the *Concrete* sciences which *are* conceptual descriptions of phenomena. In other words,† the abstract sciences deal with modes of perception, the concrete sciences with the contents of perception.

For the most detailed map of science as yet worked out, we may refer to the concluding chapter of Karl Pearson's *Grammar of Science,* noticing only: (1) that it has been almost unanimously recognised as convenient that the sciences dealing with organisms (Biology, Psychology, Sociology) should be distinguished from those which deal with inorganic phenomena (Chemistry and Physics); and (2) that different departments are bound together, e.g., applied mathematics linking the abstract to the concrete, chemical physiology linking the study of the inorganic to that of the organic.

Thus, the broad lines of the scientific map may be represented in a scheme like this:—

* Karl Pearson, *Grammar of Science,* rev. ed., London, 1900, p. 513.

† Ibid., p. 515.

ABSTRACT AND CONCRETE SCIENCES.				
LOGIC. [METHODS OF DISCRIMINATION GENERALLY.]	Links between abstract and concrete.	SOCIOLOGY PSYCHOLOGY BIOLOGY ...	 Botany, Zoology, etc.	Links between concrete.
MATHEMATICS.		CHEMISTRY AND PHYSICS.	Astronomy, Geology, Meteorology, etc.	

THE CORRELATION OF KNOWLEDGE.

Verworn speaks of Johannes Müller (1801–1858) as "one of those monumental figures that the history of every science brings forth but once. They change the whole aspect of the field in which they work, and all later growth is influenced by their labours." When we enquire into the secret of Müller's achievements, we find that he combined genius with unsurpassed working-power, but it is important to notice more definitely what we may call his sense of the correlation of knowledge. "He did not recognise one physiological method alone, but employed boldly every mode of treatment that the problem of the moment demanded. Physical, chemical, anatomical, zoological, microscopic and embryological knowledge and methods equally were at his disposal, and he employed all of these whenever it was necessary for the accomplishment of his purpose at the time." *

If we take Pasteur (1822–1895) as another representative figure in nineteenth century science, we may

* Max Verworn, *General Physiology*, trans. 1899, p. 20.

read the same lesson. Far from being pre-occupied with vivisection and inoculation, as the commonplace summary too often suggests, he passed in an ever-widening spiral of scientific investigation from his rural centre upwards, from tanpit to vat and vintage, from manure heaps, earth-worms, and water-supply to the problems of civic sanitation. On each radius on which he paused he left either a method or a clue, and set some other enquirer at work. Biologist and brewer, chemist and physician, agriculturalist and surgeon,—and how many more—have all felt the influence of his achievements, and part of the secret of these lay in his sense of the correlation of knowledge, in his grasp of the fact that workers in different departments of science have much to say to each other.*

Another, and again a different illustration may be found in the work of Darwin. It was natural that one who discerned so vividly the correlation of organisms should also realise the correlation of knowledge. We see this, for instance, as we turn over the pages of *The Origin of Species, The Descent of Man, Variation under Domestication,* and his other great works, and infer from the foot-notes something of the range of the fields in which he gleaned. We see it in his recognition of the far-reaching scope of the doctrine of descent, that it belongs not merely to the biologist, but affects psychology and sociology, the whole life of man and society. He once expressed satisfaction that he had not been permitted to become a "specialist"; it is hardly too much to say that there is no specialism in concrete organic science which he has left unaffected.

* P. Geddes and J. Arthur Thomson, "Louis Pasteur," *Contemporary Review*, Nov., 1895, pp. 632–644.

Let us take an illustration from the history of astronomy. Apart from pioneer suggestions, astronomy was till the middle of the century a science descriptive of the movements of the heavenly bodies. But the establishment of spectroscopy by Kirchhoff and Bunsen was the beginning of a close correlation between astronomy and other sciences. Formerly "it was enough that she possessed the telescope and the calculus. Now the materials for her inductions are supplied by the chemist, the electrician, the enquirer into the most recondite mysteries of light and the molecular constitution of matter. She is concerned with what the geologist, the meteorologist, even the biologist, has to say; she can afford to close her ears to no new truth of the physical order. Her position of lofty isolation has been exchanged for one of community and mutual aid." *

NEED FOR CRITICISM OF SCIENTIFIC WORK.

A large part of the scientific work done year after year is instinctive and spontaneous rather than deliberate and controlled. It is done because the doers have delight in it, a "natural taste," as they say, and thus self-criticism as to the value of it is silenced. The result is an enormous waste of mental energy. Big-brained men often fritter away their intelligence on the study of trivialities, which may be admirable as what used to be called an "elegant amusement," but represents a great loss to science.

It is perhaps useful at times to stand by and calmly watch the succession of gifts laid upon the altar of science. There are the well-finished offerings of those who have what seems to some of us so in-

* A. M. Clerke, *History of Astronomy in the Nineteenth Century*, 1885, p. 183.

estimably precious—the leisure to work thoroughly undisturbed; there are the ill-finished offerings of the impetuous, and enthusiastic, and hard-driven; there are humble offerings which have involved years of self-denial; there are brilliant offerings which have meant but a few flashes of clear insight; there are tarnished offerings which have been gained illegitimately; there are heroic offerings which are received *in absentiâ* from those who have died to know; there are epoch-making offerings, like those of Newton or of Darwin, which set the whole altar aflame.

One cannot see this vision of the altar of science without being impressed. There is a majesty in the advancement of knowledge, and a sublime patience in research. But it is difficult to tell how much of the work would be regarded as effective expenditure of energy by a sufficiently wise judge, wise for science and wise for humanity. The only sufficiently wise judge is Time, whose decisions are often very slow. That contemporary appreciation of an offering has often been far from just is one of the most obvious facts in the history of science.

But as one lingers near this "altar of science," one must be much absorbed if one does not hear a murmur of dissentient voices. The practical man growls over the time spent in the classification of seaweeds when "what we want is more wheat," over embryological research instead of fish-hatching, over the theoretical puzzles of geology instead of the search for more coal and iron. When the practical man supports the scientific worker, he has doubtless some right to control the direction of his activities, though it is not very clear that much good has ever come of this. Man does not live by bread alone, and some of the most important practical

results, such as the use of antiseptics, have been reached by very circuitous paths. It did not seem a very promiseful beginning which Pasteur found in the study of tartrate crystals, and yet what a beginning it was!

It is long since Bacon replied to the objection of the practical mood which we have just noted. We may recall his vindication of investigations which are light-giving (*lucifera*) against those which are of direct practical utility (*fructifera*); and the deliverance "Just as the vision of light itself is something more excellent and beautiful than its manifold uses, so without doubt the contemplation of things as they are, without superstition or imposture, without error or confusion, is in itself a nobler thing than a whole harvest of inventions."

But there are many other dissentient voices. The humanitarians mutter "cruelty," "inhuman curiosity," "barbarous inquisitiveness," "triviality." The scholars say with a smile, "We would rather know the thoughts of Plato and Aristotle than pore over the entrails of an antediluvian frog,"—"a Kindergarten study at the best is your Natural Science." The poets and artists laugh and say, "Grubbers among dust and ashes, besmirching the wings which might lift you as eagles," "a botany which teaches that there is no such thing as a flower," "a biology which has become necrology," "a chemistry which has flooded the world with aniline dyes," "a physiology which has made a debased—not kailyard, but midden-heap—literature possible," and so on.

These and a hundred other criticisms reach the ear, and though a retort may readily be made to each, the feeling remains that there is some justice in most of them, that scientific industry is not always suf-

ficiently self-critical. To rise above particular criticisms to a general basis of criticism would be a great gain, and perhaps this may be found in a recognition of what may be called *The Three Unities.*

UNITY OF LIFE.

The first of these unities is the Unity of Life. We have already referred to the three main moods or attitudes of mind observable in human relations to nature—practical, emotional, and scientific. They find expression in doing, feeling, and knowing; in practice, in art, and in science; they may be symbolised by hand, heart, and head.

We are not of course supposing the existence of altogether separable faculties, or nonsense of that sort; we do not say that there are any purely practical, or exclusively emotional, or solely scientific men; we simply note what appears to be a fact of life that we can practically distinguish around us the doers, the feelers, and the knowers. And as one of the moods often has temporary dominance, we are all apt to err in over-doing, *or* over-feeling, *or* over-knowing.

It is believed by most comparative physiologists that the ears of many of the simpler animals are not hearing ears, but rather directive organs, important in balancing, equilibrating, and orientation. It is such an equilibrating organ that we all need to help us to adjust the balance of our moods.

Our thesis then is that some measure of completeness of life—in ideal at least—is the condition of sanity in human development. A thoroughly sane life implies a recognition of the trinity of knowing, feeling, and doing. It spells health, wholeness, holiness, as Edward Carpenter has said.

Contrariwise, non-humane activity, whether practical, emotional, or scientific, implies primarily a denial of the trinity referred to, a violence to the unity of life. The one-sided man has let at least two of the lights of life die out.

To be wholly practical is to grub for edible roots and see no flowers upon the earth nor the stars overhead; to be wholly emotional is to become unreal and effervescent; to seek only to know is to deny our birth-right and birth-duty as social organisms.

The various sins of our relations to nature—sins of ignorance, indifference, irreverence, cruelty, obscurantism, and so on—all imply some denial of the trinity.

Science for its own sake requires to be continually moralised and socialised, oriented, that is to say, in relation to other ideals of human life than its own immediate one of working out an intellectual cosmos. Our science requires to be kept in touch at once with our life and with our dreams; with our doing and with our feeling; with our practice and with our poetry. Synergy and sympathy are needed to complete a synthesis.

If the above be a reasonable position, it suggests that the scientific way of looking at the world is not the only one. There are many whose outlook expresses quite a different mood. As we have seen, the student of science does not pretend to *explain* the order of nature, he simply tries to re-describe it in general conceptual formulæ, and he believes that his task is justified by the results—intellectual, emotional, and practical. He has a right to insist on being heard as to the aim of his own industry, but it does not follow that his statements are of equal value when he speaks of other than scientific

expressions of the developing human spirit. Irritated by the way in which others misunderstand him, he often misunderstands them. Thus as an expression of the recoil of the scientific mood from metaphysical speculation—a recoil which seems to us largely due to misunderstanding of aims—we may quote what Liebig said of Schelling: "I myself spent a portion of my student days at a university where the greatest philosopher and metaphysician of the century charmed the thoughtful youth around him into admiration and imitation; who could at that time resist the contagion? I too have lived through this period—a period so rich in words and ideas and so poor in true knowledge and genuine studies; it cost me two precious years of my life." *

The above citation expresses the opinion of many scientific workers, and yet is it not, to say the least, arrogant to attempt to ignore the attempts which have been made throughout all the ages to re-express the order of nature in transcendental or metaphysical terms? "The search after ultimate causes," says Dr. Merz, "may perhaps be given up as hopeless; that after the meaning and significance of the things of life will never be abandoned: it is the philosophical or religious problem."

We cannot readily understand a phenomenon which seems to occur—that of an active and well-disciplined brain in which there are, so to speak, idea-tight compartments, the contents of which are prevented from mutual influence. The mental like the bodily life should be a unified system of correla-

* *Ueber das Studium der Naturwissenschaften. On the Study of the Natural Sciences*, 1840, cited by E. von Meyer, *History of Chemistry*, 1891.

tions. It cannot be normal that a man should cherish incompatible ideas. But that is not to say that he may not be both scientific and metaphysical, or both scientific and poetical. These are indeed different moods, but complementary rather than incompatible, and disharmony results only when they are allowed to mix with one another in verbal statements, or when the particular concrete expressions given to the poetic or philosophic activity happen to be at variance with sound science. Between the moods there is no variance. The different moods express different ways of looking at things, and use as it were words of different languages. The evolutionist postulates a beginning *somewhere*,—an initial order of nature instituted in some fashion quite unknown and implying the potentialities of the future in some fashion quite unknown; the creationist gives in non-scientific or transcendental terms some account of the institution of the order of nature; the ideas are not antithetical, they are incommensurable. Moreover, if we may take another point of view for a moment, the teaching of the history of science leads us to a strong feeling of gratitude to the deductive or *a priori* thinkers. They were at least thinking—often with a broad perspective—and that cannot always be said of researchers. They may have interpolated fanciful ideas where facts alone are decisive, their deductions may have led induction off the scent, they may have blinded vision by preconceptions and deranged reasoning by prejudices, they may have caused confusion by mixing up objective and subjective terms, and done many other evil things; but it is a historical fact that astrology led on to astronomy, alchemy to chemistry, cosmologies to geology, and superstitious medical lore to

physiology. Even the frequent break-downs of the *a priori* methods prompted *a posteriori* enquiry.

UNITY OF SCIENCE.

The second unity—a recognition of which makes for sanity—is the unity of science or knowledge. The sciences in the broadest sense form *one body of truth.* Blocked apart for practical convenience, treated of in separate books, expounded by different teachers, investigated in different laboratories, they are parts of one discipline, illustrations of one method, expressions of one mood, and attempts to make clear—if never to solve—the one great problem of the Order of Nature. The sciences have their ideal completeness only when inter-related. This is the ideal alike of the philosopher's stone, of the encyclopædic movement, and of the most modern scientific synthesis.

This note of the unity of the sciences is sounded—though so often quickly silenced—in the word University. Its value is demonstrated by the history of the sciences, which shows how often a fresh contact between two departments has led to great advances. It becomes insistent when we consider a big subject like the physiology of marine organisms, which there is no hope of understanding except through the combined efforts of chemist and physicist, botanist and zoologist, meteorologist and geographer.

Whether we take a hint from the term "Natural History," or from the word "Organisata," which Linnæus used to include both animals and plants, or from Comte's hierarchy of the sciences, or from Caird's essay on the unity of science, or from Spencer's *Synthetic Philosophy*—we have purposely chosen incongruous examples— we hear the same note

of unity. It is the end towards which our teaching and learning must move, even if the curve be asymptotic.

As we have already noted, the study of living creatures stands midway between the chemical and physical sciences, which are in a sense beneath it, and the mental and social sciences, which are in a sense above it; there are lights from below and lights from above; and to attempt to shut out either means unnecessary obscurity. The living organism is a synthesis, whose secret has certainly not been solved, but we are surely saved from some misunderstandings of it by the results of other sciences than Biology.

Thus, there comes to the aid of the biologist or any other scientific worker, this criterion: Am I, as a thinker, teacher, and investigator, recognising, respecting, doing no violence to, *the unity of science?* Am I recognising other disciplines, other bodies of thought, as I wish that they should recognise mine? Even more positively, the criterion might read: Does this piece of work in any way tend to the realisation of the Unity of Science?

UNITY OF NATURE.

A third unity may perhaps be spoken of as *the unity of nature*—by which we mean to refer both to the unity of the particular subject of scientific enquiry, and to the unity of the whole system of things. To the psychologist, the unity which must not be lost sight of is that of the personality which he is studying. To the biologist, the unity which cannot be ignored without fallacy is the unity of the organism. But besides these there is the unity of the whole system of nature in which

part is linked to part by sure, though often subtle bonds in which nude isolation is as rare as a vacuum. In regard to all matters we have many questions to ask, each difficult, each interesting, each often requiring special methods of investigation, and in the search of answers we are sometimes apt to forget the unity of the subject. There can be no doubt, for instance, that in the eager pursuit of comparative anatomy, or chemical physiology, or any other particular line of biological enquiry, the unity of the organism is often forgotten. The same is true, though perhaps less markedly, in other sciences, where the fascination of some one aspect or method causes the investigator to lose his sense of the unity of his subject. Specialism of enquiry is necessary and valuable, but it loses its virtue if the specialist remain like a beetle in a rut, the sides of which form the horizon.

Thus we reach a third criterion of scientific work and thought; we must force upon ourselves the question—Am I studying this—whatever it is—as I would have myself studied, as a whole, as a unity, and moreover as a part in the great system of things which we call Nature, which is also a Unity?

To sum up, there are a certain number of 'isms which we scornfully call fads. They express a loss of perspective,—intellectual, emotional, or practical, the dominance of some fixed idea which distorts or obscures vision. It is easy to scoff at one or other of these fads, but the chances are that we are ourselves victims. It is more in the line of progress to study their meaning, and then we see that they are often reactions against some denial of the unity of life, the unity of science, the unity of nature, or some greater unity than these.

CHAPTER III.

Progressiveness of Science.

THE FIRST CONDITION OF SCIENTIFIC PROGRESS.

No one who has watched a colony of ants with any precision will find it easy to agree with the ancient proverbialist that the "little people" are "exceeding wise," *if* we mean by "wise" to imply anything like "knowing" or "scientific" in the human connotation of these terms. Ants are marvellous creatures of routine, but they are foolish before the new. Their little complex brains are well-stocked compendia of ready-made nervous mechanisms, but they are eminently non-educable. It is very difficult to prove that the little people are able to profit by experience at all. Therefore, if one were inclined to give a lifetime to the education of insects, one would not begin with ants. Their brains are too much "set," or stereotyped, to be readily docile. It would be unwise to be dogmatic regarding this difficult problem, but the general verdict of present biological and psychological research on the behaviour of ants is, that their marvellous powers are not acquired by the individual in relation to the particular needs of its life, are not readily modifiable to suit novel contingencies even of a simple kind, are not, in the strict sense, intelligent, but are hereditary instincts which have arisen in the course of a long series of generations by the action of natural selection on germinal variations.

If a disaster befell the ant-hill and reduced the

community to the minimum number necessary to avoid extinction—say to a fertile queen with two or three workers to look after her—there seems no reason to doubt that in a short time the whole ant-hill would contain a population as effective as before. Their powers are implied in their brain-inheritance; their capabilities of effective response to their environment have little or no external registration.

It is possible that in some animals, where a social life is sustained generation after generation, there may be something corresponding to tradition which gradually grows larger in its content, which forms what may be called an external heritage as contrasted with a natural or organic inheritance.

It is also to be noted that some of the higher animals seem to have words—particular sounds indicative of certain things or expressive of definite emotional states—and it can hardly be doubted that the existence of these will facilitate mental processes. In some cases, too, the permanent products which animals make—dwellings, nests, roads, and the like—may become suggestive symbols, and may be of some importance as stimuli to successive generations.

Yet after all these admissions are made, it remains as a great contrast between man and animals that our possession of language and methods of recording conclusions makes the progress of science possible. In the case of ants it seems as if the brain had evolved in the direction of a more and more perfect automaton; in the case of man, the existence of external means of registration has made it possible for the brain to be born more and more plastic, less weighted by an inheritance of ready-made powers, in a word, more educable. "To the educable animal—

the less there is of specialised mechanism transmitted by heredity, the better. The loss of instinct is what permits and necessitates the education of the receptive brain." *

In this book-ridden age when the student so often laboriously uses another's eyes instead of lifting his own, and when many, as a stern critic has said, "seem unable to cerebrate except in the presence of print," the hasty wish has sometimes been expressed that all books could be burned. But, however, interesting the century succeeding the conflagration might be—with enthusiastic reconstructing of the classics from reminiscences and with uninhibited independence of inquiry—it is probably safe to say that men would return to the conclusion which we are now expounding, that the first condition of the progressiveness of the sciences is in permanent methods of external registration. Extraordinary, indeed, would be the calamity if the Temple of Science should fall like the Tower of Babel, if all the living embodiments of science should suddenly disappear, if all the instruments and inventions which are suggestive symbols of hard-won generalisations should be lost, if all the phrases which condense discoveries into formulæ should be wiped out of human language—then we should have to begin at the beginning again. The prime condition of the progressiveness of science is in external modes of registration,—in words and formulæ, symbols and instruments.

THE FACT OF PROGRESS.

In an eloquent lecture on "The Progressiveness of the Sciences," the late Principal John

* E. Ray Lankester, *Nature*, LXI., 1900, p. 625.

Caird spoke as follows: "The history of human knowledge is a history, on the whole, of a continuous and ever-accelerating progress. In some of its departments this characteristic may be more marked and capable of easier illustration than in others. External accidents, affecting the history of nations, may often have disturbed or arrested the onward movement, or even, for a time, seem to have altogether obliterated the accumulated results of the thought of the past. But on the whole the law is a constant one which constitutes each succeeding age the inheritor of the intellectual wealth of all preceding ages, and makes it its high vocation to hand on the heritage it has received, enriched by its own contributions, to that which comes after. In almost every department of knowledge the modern student begins where innumerable minds have been long at work, and with the results of the observation, the experience, the thought and speculation of the past to help him. If the field of knowledge were limited, this, indeed, would, from one point of view, be a discouraging thought; for we should in that case be only as gleaners coming in at the close of the day to gather up the few scanty ears that had been left, where other labourers had reaped the substantial fruits of the soil. But, so far from that, vast and varied as that body of knowledge which is the result of past research may seem to be, the human race may, without exaggeration, be said to have only entered on its labours, to have gathered in only the first fruits of a field which stretches away interminably before it." *

It is one of the aims of this volume to illustrate

* A lecture delivered in 1875. Reprinted in *Lectures and Addresses*, 1899.

the progress of the sciences within a century, and there are many ways in which the impression of progressiveness may be made vivid. Many of the articles in the older Encyclopædias are splendid pieces of intellectual workmanship, but to read one of them and then its correspondent in a modern encyclopædia is like a sudden transition from an incipient spring to midsummer. And yet we know that, to our successors, this modern article will soon seem quite vernal.

There have been scientific works like those of Aristotle, Pliny, and Galen which lasted in varied forms through centuries; and there are masterpieces, like the books of Euclid, and Newton's *Principia,* which in some form will be text-books while learning lasts; but every one knows that nowadays even the best of text-books is very short-lived.

If we take a survey of the sciences, from astronomy to sociology, how striking are the changes, alike as to facts and ideas, in the last hundred years. He must be indeed *blasé* or callous who does not feel exhilaration in the thought of the advance in the interval between Laplace and Lockyer; between Count Rumford and Lord Kelvin; between Hutton and Playfair and the Geikies; between Richard Owen and Louis Agassiz on the one hand, Cope and Zittel on the other; between Cuvier and Huxley; between Lamarck and Ray Lankester; between Von Baer and Francis Balfour; between Bichat and Sir Michael Foster; between Erasmus Darwin and his grandson; between Reimarus and Romanes; between Prichard and Taylor; between Adam Smith and Herbert Spencer. To any one who knows even a little concerning the history of science the contrasts of these coupled names must stimulate afresh the impression

that there are few facts more marvellous and inspiring than the advancement of science.

ITS NECESSITY.

The primary reason for the progressiveness of science is simply that the scientific mood is a natural and necessary expression of the developing human spirit. It may be thwarted, discountenanced, even banned, as it was during the early mediæval centuries, but stifled it cannot be. The innate inquisitiveness, the passion for facts, the active scepticism, the desire after lucidity, and the other qualities to which we have referred as characteristics of the scientific mood, may be widespread or confined to small circles of enquirers, may be exhibited in regard to all orders of facts or restricted to a single department, but the scientific mood is essential to man's nature, and science will not cease to progress until both practice and poesy have likewise come to an end.

There is no doubt that many pieces of scientific research are entered upon with the set purpose of solving practical problems; on the other hand much scientific activity is as spontaneous and instinctive as a great part of artistic activity is: in other words, it is a natural expression of the man. In evidence of this, at a time when the pursuit of science is so often a "profession" and a "*Brodwissenschaft*," one may recall that up and down through the country one finds many obscure enthusiasts pursuing in their leisure hours, or in hours when others sleep, some path of scientific enquiry—astronomical, geological, botanical, zoological, or otherwise—in most cases without hope of or wish for reward, without desire

for publicity or publication, for they are genuine amateurs in the literal sense.

Another way of illustrating the ineradicable scientific mood is to consider a few biographies of eminent workers, and to notice how often the environmental conditions were the very reverse of propitious. The "Pursuit of Knowledge under Difficulties" is a well-worn theme,—of considerable interest to those who have had experience in the task of trying to induce uninterested students to pursue knowledge under the most favourable conditions.

It may perhaps be argued that although the scientific mood is characteristically human and must therefore persist, while man as we know him does, yet the subjects of enquiry are limited and the range of our sense-experience is not infinite. Therefore there must be an end to the progress of science, and a time must come when the confession *ignoramus* will be no longer heard in the land, for all the problems that have not been solved will be insoluble, and *ignorabimus* will remain as the only word of intellectual modesty. It can hardly be said that this question of the completion of scientific enquiry is one of practical politics, but it may not be unprofitable to consider it for a little.

It was surely a momentary aberration which led a great zoologist to suggest not long ago, in the enthusiasm of a retrospect, that it was now about time for us to be making a list of the things we did *not* know. A very different suggestion was made in a remarkable sentence in the presidential address delivered by the late Dr. Edward Orton at the 1899 meeting of the American Association for the Advancement of Science. "The founders of the Association, fifty years ago, clearly saw that they were

in the early morning of a growing day. The most unexpected and marvellous progress has been made since that date, but as yet there is no occasion for, and no prospect of modifying the title (Association for the *Advancement* of Science). We are still labouring for the advancement of science, for the discovery of new truth. The field, which is the world, was never so white unto the harvest as now, *but it is still early morning on the dial of science.*" It is this last sentence which should be pondered over by any one who is inclined to speak or think or act as if it were already late afternoon!

The fact is, that to whatever department of scientific enquiry we turn, we find an embarrassment of unsolved problems. Everywhere there is a widening outlook, a more and more intensive analysis, but never a hint of finality. Everywhere we hear the words, "for leagues and leagues beyond, and still more sea." It might seem to some that an old-established and persistently prosecuted department of science like human anatomy must be now almost exhausted, but among the experts the suggestion would be received with derision. It might seem to some that a little animal like the lancelet, every millimetre of whose body has been subjected to the scrutiny of the keenest zoological observers, must be now almost completely known, but the suggestion is one that only an outsider could make. We have not nearly finished with this one animal, and is it not a little one? The animal cell has been studied with the most assiduous carefulness, with gradually perfected microscopes, with ingenious devices of fixing and staining and cutting, for more than three-quarters of a century, and yet it remains very imperfectly known. We may recall, for instance, that the dis-

covery of the central corpuscles or centrosomes—somewhat enigmatical, apparently very important, and practically constant components of the animal cell—members of the "cell-firm"—dates from only a few years ago.

Nor should it be forgotten that we live in a world of change, in which a process of evolution is going on, and that, therefore, the subject-matter of a science is developing just as the science is. We hear of stars that die and of others that are a-making (we may use the present tense though the events are, of course, vastly older than our observation of them); even in a human lifetime—the minutest moment compared with the earth's age—the features of a countryside may change perceptibly, indeed a shore may get a new face in a single storm; the distribution of plants and animals is in process of rapid flux; the characters of organisms, including ourselves, are slowly but surely changing. Thus with an evolving subject-matter before our eyes, we need say little about the prospect of—completed science.

SCIENTIFIC CONCLUSIONS OF THE FIRST MAGNITUDE.

We hear so much nowadays in regard to the rapid progress of science that there seems some danger lest our impression become exaggeratedly sanguine. In more critical moods, however, the suspicion arises that in spite of the rapid accumulation of natural knowledge, information often proves itself the death of wit; and that in spite of the remarkable diffusion of the scientific mood throughout wide circles in our community, the growth of scientific insight is really very slow.

That this suspicion is not unfounded becomes clear

when we consider the small number of scientific generalisations which we can venture to describe as of the first magnitude. We begin to count these: The doctrine of the indestructibility of matter, foreseen by Democritus, but for practical, scientific purposes only about a century old—dating from Lavoisier; the doctrine of the conservation of energy, with its corollaries of transformability and dissipation; the theory of gravitation, with its far-reaching applications; and the theory of organic evolution which will be linked for ever with the name of Charles Darwin.

But after we have enumerated these, we begin to hesitate. Are there any others on the same plane, which thoughtful men accept without hesitation and without saving clauses, to lose any of which would spell intellectual disaster? Should we include, for instance, what is grandiloquently called the Law of Biogenesis—which states that, so far as we know, every living creature has its parentage in another living creature or in two other living creatures? This is a big fact, no doubt, but it is hardly more than an empirical fact, and there are many who suppose from foreshadowings which they see that the coming events of the next quarter of a century will convince us that this at present unimpeachable conclusion will be shown to be fallacious, not in itself perhaps, but in its suggestion of an impassable gulf between the not-living and the living. Or should we include the "*biogenetisches Grundgesetz*"—the Recapitulation Doctrine—that the individual development recapitulates the racial evolution, or that the organism in its becoming climbs up its own genealogical tree? but there are many who will agree with Mr. Sedgwick—the eminent zoologist of Cam-

bridge—that before this recapitulation doctrine can be accepted it must be subjected to emendations so serious that it comes to resemble a shoe cobbled so often that almost nothing of the original structure remains. We read of the stuffed horse of Wallenstein at Prag which has "*only* the head, legs, and part of the body renewed," and the "*biogenetisches Grundgesetz*" seems much in the same state at present. In revised form it must prove its power of survival a little longer, before we can admit it to a place of honour among the scientific generalisations of the first magnitude.

A recent paper on the cardinal principles of science reminds us that we have overlooked "The Uniformity of Nature," which states that in similar conditions similar things are likely to happen, and also the platitudinarian doctrine of "The Responsivity of Mind," which asserts that minds react in similar ways to similar stimuli. With every wish to be generous, we cannot throw these in, for the first seems a platitude—a fallacious platitude—and the second, well, it is a corollary of the first.

Huxley gets credit for the phrase "The Uniformity of Nature," which has been called a cardinal principle, indeed *the* cardinal principle of science. But if Huxley made the phrase, which we doubt, it does not seem so happy as some others that he minted. It is difficult to state clearly what the so-called principle means. That there are uniformities of sequence in the world around us all will admit, —else there were no science possible—but what *the* uniformity is remains obscure. We believe that the gravitation formula fits wherever it can be applied, that is one uniformity; we find no evidence to warrant our doubting that what we call matter and

energy always persist however their forms of expression may change, here are two other uniformities—or, perhaps, the two are one; but there are not many other conclusions which admit of the same universality of application and verifiability of accuracy. We know the "law of biogenesis," *omne vivum e vivo,* to mean that, so far as our experience goes, every living creature springs from some other living creature; we do not know of any exception to the statement, but we see no warrant in this for asserting that the so-called law was, or will be, or even is always true. And the same doubt, which becomes more assertive when we consider this last instance, is not silent even in regard to the alleged indestructibility of matter or the alleged indestructibility of power. It does not seem particularly forcible to retort that "one cannot conceive of the reverse happening," for it is not so long since a belief in spontaneous generation was widespread, or since the idea that the earth was not the hub of the universe was deemed by many—and these not small-brained men—"quite inconceivable." And these were the very words of Mother Grundy when she first heard of the Doctrine of Descent.

In short, is there not a radical fallacy in the phrase "The Uniformity of Nature," since our so-called natural laws are only descriptive formulæ of what is seen and known in given conditions of space and time, neither "governing nature," nor "explaining nature"? As descriptive formulæ of observed phenomena, presumably descriptive of *similar* unobserved phenomena, they make it easier for us to look out upon the world without intellectual biliousness—indeed with the greatest of joy, to follow the course of events with some appreciation of their orderliness,

to utilise them for our practical purpose; but, surely, it is time that we ceased supposing that they enable us to explain, to see the ultimate causes, the "real inwardness," of what we observe.

But even if the reiterated distinction between descriptive formulæ and explanations be not admitted —its vindication will be found in Karl Pearson's *Grammar of Science*,—it may perhaps be granted that the less we say about the Uniformity of Nature the better for the consistency of our scientific mood.

Is not the whole point expressed in Bacon's aphorism?—"Man, as the minister and interpreter of nature, does and understands as much as his observations on the order of nature, either with regard to things or the mind, permit him, and neither knows nor is capable of more." It is difficult, perhaps, to say what the word "understand" means in this aphorism, but if it mean "redescribe in simpler terms," it expresses our present position.

There is another consideration which should perhaps give us pause in our talk about the Uniformity of Nature. It may be illustrated by the following quotation from a paper by Winkler.*

"Four hundred years ago Nicholas Copernicus left, as a young master of philosophy and of medicine, the old university of Ulica St. Anny, at Cracow, to go to Bologna and to Rome for the purpose of consecrating his talents as a mathematician to the study of astronomical sciences. There, attacking the enigma of the firmament, he finally attained the certainty that the earth was not, as had been hitherto believed, a central fixed world, but a sphere suspended freely in space, a planet similar to the other planets,

* Transl. in *Rep. Smithsonian Inst.* for 1897, pp. 237–246.

turning around the sun and having a movement of rotation around its own axis under the action of gravitation. It was, indeed, a true revolution in the theories that had been hitherto held, this theory that fixed the sun in the firmament in spite of its daily ascent and disappearance; an idea that, at the present day, has become familiar to us. And further, we now know that neither is the sun itself fixed, but that it is drawn with all its cortege of planets along a course without end, across space without limit. Whence comes it and whither goes it? Properly speaking, we know nothing about it, and doubtless we will never know either its origin or its end; but as the earth turns around this movable sun, it hence results that our planet does not describe a closed path, but a sort of spiral, and that it never returns to a spot that it has once quitted. Each second takes our planet to a new point in the universe, and from this incessant displacement it ought to follow that no phenomenon or event can ever reproduce exactly any anterior phenomenon. Clouds may resemble each other, as one sunrise resembles another, but there is never an absolute coincidence, and it would seem that these variations ought to be perpetuated throughout the course of time that is embraced by the history of humanity.

"It would be useless to push further these considerations, they are merely speculations; but they lead to this thought, which, although unsupported, continually recurs to our mind—the possibility of a progressive transformation of matter in a given direction, in that they show that everything that is with us is drawn along in a dizzy course across an unknown immensity."

Let us return, however, to our particular point

in this section, which was the small number of scientific generalisations of the first magnitude.

What, some one may indignantly ask, what of the atomic theory, the periodic law, the kinetic theory of gases, the mechanical theory of heat, the undulatory theory of light, the cell-theory, Weber's law, and so on? To which we would answer that while these are doubtless of importance, they lack the generality and the intellectual influence of the four great generalisations already mentioned—the indestructibility of matter, the conservation of energy, the formula of gravitation, and the theory of organic evolution. What impresses one then is, that scientific generalisations of the first magnitude are few, and therefore that the scope for progressive science has at present no visible boundaries.

FACTORS IN FURTHER PROGRESS.

(*a*) *Growing Intensity of the Scientific Mood.*—It cannot be doubted that serious scientific study is now common in circles where half a century ago it was rare; this means an increasing body of observers, critics, and formulators. It is also certain that scientific methods are now being applied to orders of phenomena which half a century ago were observed and discussed in a very easy-going and light-hearted fashion. Some one has said rather bitterly that every science must pass through three periods: of presentiment or of faith, of sophistry, and of sober research; but it may be confidently asserted that most departments of science have now entered upon the third period.

It is not long since comparative psychology was,

apart from a few classical works, for the most part anecdotal. Precision of observation and record was blurred by fancies; facts and inferences from facts were subtly intermingled; experiment was almost unknown, indeed scarcely thought of; and transcendental preconceptions prejudiced the whole outlook. But these blemishes are rapidly disappearing, and we see the rise of a young science,—careful, painstaking, precise, given to measuring and experimenting.

To take another illustration. It is well known that one of the master-keys to evolutionist problems is labelled "*variation*," by which is usually meant the process or the result of innate or constitutional change which renders a living creature from birth onwards more or less different from its parents. Since the process of variation furnishes a great part, if not the whole, of what may be called the raw material of progress, its importance is obviously fundamental. And yet the post-Darwinian history of biological activity in reference to variation has only recently begun to be creditable to science.

Let us quote a few sentences from Mr. Bateson's *Materials for the Study of Variation* (1894)—a work which has done much to lift our feet out of the mire. "We are continually stopped by such phrases as, 'if such and such a variation then took place and was favourable,' or, 'we may easily suppose circumstances in which such and such a variation if it occurred might be beneficial,' and the like. The whole argument is based on such assumptions as these—assumptions which, were they found in the arguments of Paley or of Butler, we could not too scornfully ridicule. 'If,' say we with much circumlocution, 'the course of Nature followed the lines we have

suggested, then, in short, it did.' That is the sum of our argument. . . . Surely, then, to collect and codify the facts of Variation is the first duty of the naturalist. This work should be undertaken if only to rid our science of that excessive burden of contradictory assumptions by which it is now oppressed. . . . If we had before us the facts of Variation there would be a body of evidence to which in these matters of doubt we could appeal. We should no longer say '*if* Variation take place in such a way,' or '*if* such a variation were possible'; we should on the contrary be able to say, 'since Variation *does*, or at least *may* take place in such a way,' 'since such and such a Variation *is* possible,' and we should be expected to quote a case or cases of such occurrence as an observed fact."

It was in this mood that Bateson compiled his invaluable work, which, though still represented by only Part I., has been a big stride towards a more scientific basis for the study of organic evolution. It has been followed by numerous statistical studies of actually occurring variations, by experimental attempts to distinguish between germinal variations and bodily acquired modifications (due to the influence of functions and environment), and so on. The point is, that here, as in many other cases, an over-impetuous, undoubtedly too easy-going science, has had to retrace its steps, and to begin again where science always begins, in precise and unprejudiced observation and recording of facts, in measurement, and in experiment.

(*b*) *A Fuller Recognition of the Unities.*—When we recall the fact that qualitative advance is very slow, while quantitative advance is exceedingly rapid, we are led to enquire whether there may not be some

deep reason for this. Perhaps the chief reason is the limitation of human faculty which so readily leads to a disregard of what we have called the Three Unities. The limitation is partly the result of mis-education, the persistent tendency to fill the mind instead of evolving it, to set it in grooves instead of allowing it free scope. It is also due to the pressure of social conventions, which nip the buds of individuality, frown down idiosyncrasies, and allow no elbow room (*Abänderungsspielraum*) to novel variations, which are, after all, the potentialities of progress. It is also due to the pressure of the struggle for existence, which forces the young enquirer to premature specialism, that he may thereby make a name and a position for himself. "*Er will sich nähren, Kinder zeugen,*" and so on. If we may define a genius as one who has by inheritance and appropriate culture an unusual complement of powers all in strong development,—poetic as well as scientific, or practical as well as philosophical, or otherwise,—there are many facts within our experience which suggest the sad conclusion that for one genius who makes himself felt, there are perhaps nine whose light is hidden under a bushel. It is for this reason that many who are under no delusion as to the equality of men or the triumph of democracy would favour every measure which opens the portals of learning—let us say, the gates of our Universities—more widely to all sorts and conditions of men.*

There remains, however, another reason, that when the scientific student, who has retained an open and sympathetic mind, finds himself in his maturity more than ever aware of the need for correlation in knowl-

* This is now pecuniarily possible in Scotland, thanks to Mr. Carnegie's magnificent gift.

edge-making or for co-operation in science, he is also likely to find himself pre-occupied with his own problems, mastered by his strongest personal interests, burdened by immediate duties, with neither time nor energy left for that effort which an active realisation of the unities implies. For lack of sympathy in some cases, for lack of synergy in other cases, the progress of synthesis is sluggish.

For this reason we emphasise our thesis that the progressiveness of science depends first on a realisation of the Unity of Life.

The scientist, by which we mean the student of the order of nature, is incomplete in his arm-chair; he is even incomplete in his laboratory. He must be, in some measure, also a citizen, a man of feeling, and a philosopher! That even his science will suffer from his practical denial of the trinity of doing, feeling and knowing, is our argument, and this the slow progress of science seems to us to bear out. One might appeal to biologists who have because of their expert knowledge been appointed to serve on governmental commissions, dealing with practical problems of life, and ask whether, after allowing for the delay of their personal investigations, they did not return to these with new zest, widened outlook, and fresh insight. The German government dignifies prominent scientists with the title of *Geheimrath* or Privy Councillor, and in many cases there is an honour conferred, and that is all. But behind the honorary title, there is the suggestion—sometimes realised—that the expert advice thus dignified is at the service of the government in critical situations,—a plague, a famine, an exploitation of new territory and so forth. That the same sort of expert advice should be at the command of all nations who nurture

scientific academies and scientific professors, seems sound common sense, and that it would be the better for science, as well as for the community, if this were oftener called into exercise seems equally obvious.

We have illustrated our point by reference to the need for contact with the practical problems of life; but a strong case could also be made for the advantage which science would gain by endeavouring at least to understand the point of view of the artist and the philosopher.

Secondly, the progressiveness of science depends upon a fuller realisation of what we have called the unity of science. Mineralogy and petrography have acquired new vitality and greatly enhanced importance since they became definitely chemical; the method of spectrum analysis has brought astronomy from a position of isolation into intimate contact with chemistry and physics; the recent development of physical chemistry is another instance of happy and fruitful union; since physiologists called chemists to their aid physiological chemistry has become so important that what used to be relegated to an appendix in a physiological treatise now pervades the whole book; psychology has listened to biological results; and the indebtedness of social science to biology and the physical sciences is admitted by most to be of value, though the contact is still only incipient.

But while this and more may be said of actual co-operation, it remains necessary to point out that many workers, and many departments of this or the other science, continue to flounder along, whereas they might swim swiftly if they condescended to take assistance and instruction from their fellow-travellers. After all, the current is not so swift,

that there is no time for mutual consultation by the way.

Thirdly, the progress of science depends upon a recognition of the unity of the subject, which extends itself to a recognition of the unity of nature. A great part of scientific work is analytic; we take things to pieces—social institutions, man, the animal, the plant, the earth, the piece of matter—just as the boy dissects the watch. And this analysis is necessary, as well as fascinating. The danger is lest we forget that it is only a means to an end—namely, that we may put the things together again with a better understanding of the unity which we have dissolved. It is plain that in anatomy, for instance, we make an abstraction necessary for the purpose on hand, but still an abstraction—for we leave the life out of consideration. Our point is, that the analytical work of the anatomist only fulfils its function when the results are brought as a contribution towards a fuller understanding of the unity of the organism.

In the same way, to take another illustration, the comparative physiologist concerns himself mainly with an analysis of the activities or functions of organs, tissues, and cells in different kinds of creatures; and his work, still very young, has been rich in important results, and is full of promise. But, again, for purposes of research, abstractions are necessary, the living creature is abstracted not from its life—for the physiologist is always concerned with activity—but from its full life as it is lived in nature. Our point is, that physiology does not fulfil itself until its results are brought as a contribution to a fuller understanding of the life as a whole—of what is in some sense a personality with character and habits, with a complex life in a complex environment, a

member of a family, a unit in a fauna, a thread in the web of life.

And although we have taken our illustrations from biology, the same condition of progress applies to the other sciences. That man cannot be studied to much purpose, if he is persistently held in artificial isolation, is as certain as is the impossibility of understanding the earth apart from the solar system.

To sum up, three important factors in the progress of science are: a fuller recognition that science is for life and not life for science, a more practical appreciation of the benefits of co-operation between different disciplines, and a frank acknowledgment that analysis is a means not at end.

But there is another important factor; namely, the improvement of methods,—of devices by which we not only extend the range of our sense-experience but intensify our powers of precision. To give an account of the development of methods would be to write half of the history of science, and we must refer for illustration to the separate chapters of this book. But how much progress is suggested when we recall the methods of quantitative analysis in chemistry, of measuring the different forms of energy in physics, of spectrum analysis in astronomy, of microscopic technique in biology, of experiment in psychology. Apart altogether from instrumental devices, the increasing use of mathematical and statistical methods in dealing with the problems of biology furnishes a good illustration of the fact that the rate of progress is partly dependent on the methods employed.

JUSTIFICATION OF SCIENCE.

If science be a natural and necessary expression of

the developing human spirit, this is justification enough. Yet a more detailed justification may be demanded, not only by critics who object to the vast expenditure of time and money, labour and life, which the pursuit of knowledge involves, but also by those who at times lose confidence and enthusiasm, and are inclined to cry "Vanity" with the Preacher. Great conclusions are few and far between, practical discoveries bring curses as well as blessings, increase of knowledge often means increase of sorrow; and there is the endlessness of it, like that of an asymptotic line always approaching nearer a given curve but never reaching it. "Advance brings us no nearer the end of our labour, for the more we know the more we see of what remains to be known. Every problem laid at rest gives birth to two new problems which did not present themselves to the mind before." *

If we can suppose a science—Biology, for instance—arraigned before the bar of Humanity, as it should for its own sake feel itself arraigned, the lines of defence might be briefly summed up as follows: †

First, Biology is, like the other sciences, like art and poesy, a natural expression of human activity, at once a development and discipline of man. To cease to be scientific is to abdicate manhood. Along certain lines even the so-called savage is scientific.

Second: and "without prejudice," Biology is justified by practical results. In spite of many mistakes, it has made valuable contributions in relation to human health, the supply of food and other necessaries,

* Alex. Hill, *An Introduction to Science*, London, 1900, p. 41.

† See my lecture. "The Humane Study of Natural History," in *Humane Science Lectures*, London, 1897.

the use of animals, and so forth. We say "without prejudice," since we cannot, for a moment, allow that a science, as a science, should ever submit to the practical man's canon which makes immediate utility a stringent criterion of worthiness.

Third, while the partial pursuit of certain paths may sometimes have dulled or even played false to healthy emotion, the general result of Biology is to deepen our wonder in the world, our love of beauty, our joy in living. The modern botanist is, or at least ought to be, more aware of the Dryad in the tree than the Greek poet could be.

Fourth, Biology has partially worked out certain general conceptions of life and health, of growth and development, of order and progress,—centred in the idea of evolution,—which are not only attempts to see more clearly what is true, but which make for finer feeling and for the betterment of life. No doubt there have been impetuous attempts to apply immature biological results to the problems of human conduct; no doubt the sociologist has sometimes tried unwisely to force the biologist's hand; but one may still maintain with confidence that biology has justified itself in contributing to the ascent of man.

In the introduction to his *Grammar of Science,** Prof. Karl Pearson has admirably expounded the claims of science in general, and his summary may be quoted: "The claims of science to our support depend on: (*a*) The efficient mental training it provides for the citizen; (*b*) the light it brings to bear on many important social problems; (*c*) the

* The author's statement was written some years before reading the work cited.

increased comfort it adds to practical life; (*d*) the judgment."

Just as Huxley expressed himself at one with Descartes in declaring as his fundamental motive in scientific study "to learn how to distinguish truth from falsehood, in order to be clear about my actions, and to walk sure-footedly in this life," so, it should be noted, Pearson lays most stress upon the permanent gratification it yields to the æsthetic, the educational side of science: "Modern science, as training the mind to an exact and impartial analysis of facts, is an education specially fitted to promote sound citizenship. . . . This first claim of scientific training, its education in method, is to my mind the most powerful claim it has to state support. I believe more will be achieved by placing instruction in pure science within the reach of all our citizens, than by any number of polytechnics devoting themselves to technical education, which does not rise above the level of mutual instruction."

SCIENCE AND PRACTICAL UTILITY.

Science and practice act and react upon one another. On the one hand, historical enquiry shows that a science may arise out of practical lore and that it may receive fresh stimulus in every fresh application to practical problems. In gathering herbs man gathered knowledge, and in cultivating his garden he laid the foundations of the science of botany; to their gathering and gardening most teachers of botany still return with pleasure and profit. The lore of the hunter and the fisher is older than all zoology, and many will agree that the vitality of the science depends upon a periodic return to the study of the

actual life of animals as it is lived in nature. It may be going too far to say with Espinas,—"*La pratique a partout devancé la théorie,*" but there is no doubt as to the progressive impulse which comes to a science from its corresponding art.

On the other hand, an exaggeration of the importance of contact with practical problems and of immediate practical results, is, we believe, disastrous to the welfare of science, and it may not be out of place to enter a brief protest.

"The fundamental importance of abstruse research receives too little consideration in our time. The practical side of life is all absorbent; the results of research are utilised promptly, and full recognition is awarded to the one who utilises, while the investigator is ignored. The student himself is liable to be regarded as a relic of mediæval times. . . . The foundation of industrial advance was laid by workers in pure science, for the most part ignorant of utility and caring little about it. . . . The investigator takes the first step, and makes the inventor possible. Thereafter the inventor's work aids the investigator in making new discoveries, to be utilised in their turn." * In his admirable *Introduction to Science* (1900) Dr. Alex. Hill says: "Great advances have been made by investigators whose object was wholly technical. Yet, if the history of science were written, it would be found that the first step in advance, the germ of the discovery which became fruitful in the hands of the practical chemist, the mechanician, the pathologist, was discovered by the investigator, for whom science lost its interest as soon

* John J. Stevenson, "The Debt of the World to Pure Science," Pres. Address, New York Acad., February, 1898, *Science*, March 11, 1898; *Rep. Smithsonian Institute* for 1897, pp. 325–336.

as it could be put to practical use." He instances the discoveries preceding the use of antiseptics and of Röntgen rays.

Undue insistence on practical results is apt to be unjust, partly because no one is wise enough to predict the outcome of a research, and partly because secure progress in science is often extremely slow. The twitching legs of Galvani's frog were studied as a theoretical curiosity; who could have told that they pointed to the flicking needle of the telegraph? It was not for practical ends that William Smith plodded afoot over England, neither resting nor hurrying in his exploration of the strata, but how much of the exploitation of Britain's mineral resources had its origin in his maps? Or who can say that the series of discoveries which found the open sesame of coal-tar and brought forth its treasures had at first any practical outlook?

One use which a volume like this may have is to curb the impatience of the practical man in regard to experiments whose outcome he regards as useless, and to prompt him to a more generous support of scientific research. A little knowledge of the history of science may not be altogether a dangerous thing, if it suggests that from apparently inauspicious beginnings and from apparently unpromising items of honest work, great results may follow. Spectrum analysis—a method of very great importance to astronomer and physicist, chemist and physiologist—had its beginning in some apparently insignificant observations by Marcgraf, Herschel, and others. Pasteur's at first sight extremely theoretical researches on the hemihedral facets of tartrate crystals were logically as well as actually connected with his practical researches on fermentation.

Over and over again in the course of the history of science we find illustrations of the long gestation of scientific truth. Minerva-like birth is rare. "Discoveries which proved all important in secondary results do not burst forth full grown; they are, so to say, the crown of a structure raised painfully and noiselessly by men indifferent to this world's affairs, caring little for fame and even less for wealth. Facts are gathered, principles are discovered, each falling into its own place, until at last the brilliant crown shines out, and the world thinks it sees a miracle." * But it was after waiting and working for almost a score of years that Darwin published his theory of natural selection.

Another good illustration of the gradual emergence of an important conclusion is to be found in the history of the kinetic theory of gases. We usually, and rightly, associate this conception with the names of Joule and Clausius, and fix the date about 1857, but "the researches of Paul du Bois-Reymond and others have unearthed a whole list of authors who, in more or less definite ways, had resorted to the hypothesis of a rectilinear translatory motion of the molecules in order to explain the phenomena of pressure and other properties of gases. Among these Daniel Bernouilli (in his *Hydrodynamica,* 1738), seems to have expressed the clearest views, and he is usually now named as the "father of the hypothesis." †

While then we hold firmly that science is for life and not life for science, we protest against a narrow rendering of the words "for life." The practical man's impatient "What's the use of it?" often reveals

* J. J. Stevenson, *Rep. Smithsonian Inst.*, for 1897, p. 325.
† J. T. Merz, *History*, 1896, p. 433.

a vulgar materialism. "Truer relations of science to industry are implied in Greek mythology. Vulcan, the god of industry, wooed science, in the form of Minerva, with a passionate love, but the chaste goddess never married, although she conferred upon mankind nearly as many arts as Prometheus, who, like other inventors, saw civilisation progressing by their use while he lay groaning in want on Mount Caucasus." *

* Sir Lyon Playfair, Pres. Address, *Rep. Brit. Ass.*, 1885, p. 17.

BOOK TWO.

MATTER AND ENERGY.

CHAPTER IV.

A Century of Chemistry.

SEARCH FOR THE ELEMENTS.

Different Kinds of Things.—An inquisitive outlook on the world at once gives us the impression of an enormous number of different kinds of things—different in substance or composition as well as in form and activity—and we feel the need of arranging these in some order.

If we continue our inquisitive outlook we soon perceive that no small part of the apparent variety of the things we see around us is due to the fact that different stuffs or kinds of matter occur mixed up together. If we take a handful of coarse sand from the shore, we can, by working for a few hours, put it into some order, placing fragments of lime shells in one corner and pieces of quartz in another, and so on. But this sorting out is easy work, and can be done by a machine; it is not the chemist's problem,—he deals with the changes in the nature of substances which are *not* mixtures. Among these not-mixtures it is necessary to distinguish (1) a certain number of definite kinds of matter which cannot be separated by any known means into unlike parts, such as iron and

carbon; and (2) others which, by heating or otherwise, can be broken up (not sorted out) into unlike parts, such as sugar and salt. In other words, after sorting out the heterogeneous mixtures the chemist has to do with the two sets of homogeneous stuffs to which we have just referred—which are familiarly known as Elements and Compounds.

Though many of the elementary substances, such as copper, gold, iron, lead, silver, tin, zinc, sulphur, have been known from remote antiquity, the recognition of elements as such—i.e., as substances which cannot, *so far as we know at the time,* be resolved into other kinds of matter—practically dates from Robert Boyle, the author of *The Sceptical Chymist* (1680).

A hundred years later, Lavoisier, who first made the conception of elements practically useful in scientific research, enumerated thirty-three (including light and heat), but the list increased by leaps and bounds during the nineteenth century. Thus Sir Humphry Davy discovered six new metals between 1808 and 1810, and the Swedish chemist Berzelius added an equal number in about the same time. As was to be expected, the practical interests of mineralogy and metallurgy, especially in Sweden and Germany, gave zest to the search after elements, and led Scheele and others to many discoveries. By 1830, Lavoisier's list was nearly doubled, and it is still being added to.

Interactions of Elements.—Another impression that we get from our outlook is that things are changeful. We see stones weathering and crumbling, shells being dissolved away, iron rusting, coal burning, and thousands of other changes, which excite curiosity and offer problems to be solved.

A moment's reflection will show that two somewhat different sets of changes go on around us. In the frosty night water changes into ice; the sun rises, and the ice changes into water; in the bright sunshine the water may even pass into the air as vapour. Here we have one of the most familiar instances of a change of state, but the water remains in a real sense water all the time. There is no change in the nature of the stuff, and it is with changes in the nature of the stuff that chemistry has primarily to do, with the change, for instance, which occurs when, by an electric current, water is decomposed into its two constituents, hydrogen and oxygen. The chemist has as his fundamental problem, not merely the recognition and isolation of elements, but their affinity in relation to one another, their capacity of exerting chemical action or inducing chemical change.

Detection of an Element.—The question naturally rises in the mind, how does the chemist know when a given substance is an element or not; and the only scientific answer is that all substances should be assumed to be compounds until all known methods of decomposing them have been tried without success. "If the products we obtain always weigh more than the substance itself and never less, no matter to what changes it has been subjected, then, provided each change is complete and accompanied by no loss of substance through our imperfect methods, we are constrained to regard that substance as an element." *

Thus the chemical conception of an element is simply that of an undecomposed—not necessarily

* Ostwald, *Outlines of General Chemistry*, trans. J. Walker. 1890, Chap. II., "The Elements," p. 9.

undecomposable substance—since we must always bear in mind that an increased perfection of method may result in the decomposition of what was previously regarded as elementary.

Recent Discoveries of New Elements.—During the last quarter of a century the number of known elements has been very rapidly increased. In a general way, it may be said that analysis has become more penetrating, but there are several particular reasons for the increase. (1) It was by the electrolytic decomposition of alkaline earths that Davy discovered potassium and sodium; this was about the beginning of the century, and the discoverer had at his command only a feeble Voltaic pile; now intensely powerful currents are utilised, and it was by these that Moissan, for instance, was able to isolate fluorine from its combinations. (2) Spectrum analysis has shown the existence of a series of elements with characteristic spectra, and it is a remarkable fact that one of these, helium, was known from the sun before it was discovered in the earth. (3) Certain theoretical conceptions, such as Mendelejeff's periodic law, have led chemists to look out for and to find elements whose existence was predicted on *a priori* grounds. Thus Nilson in 1879 discovered scandium which Mendelejeff had foretold. Gallium, discovered by Lecoq de Boisbaudran in 1875, and germanium, discovered by Winkler in 1886, are other famous examples.

Argon.—Two of the latest additions to the list of elements deserve special notice. In 1892, Lord Rayleigh directed attention to the fact that nitrogen obtained chemically was about one-half per cent. lighter than that got from the air, and it was this minute discrepancy which led him to look for and

discover a heavier gas in the atmosphere. In the meantime, and independently, Prof. W. Ramsay discovered the same gas by removing the nitrogen by means of red-hot magnesium. Combining their results, the two investigators published their memoir on Argon, "which will go down to posterity among the greatest achievements of an age renowned for its scientific activity" (Meldola).

Argon is an extraordinarily inactive or chemically indifferent gas of great density; occurring along with atmospheric nitrogen, forming about 8 or 9 per cent. of the volume. It can be separated by incandescent magnesium or by the continued action of the electric spark, and in the latter way Cavendish seems actually to have produced it a hundred years ago! Alone or along with helium it has been found in natural waters, in minerals, and in a meteorite. It is not known to form combinations, and it does not fit in well with the periodic system, so that its real nature remains the subject of enquiry. That it is truly an element is suggested by the distinctness of its electric spark spectrum and by the discovery that the molecule is monatomic, but the possibility remains that it is a mixture of monatomic gases.

Helium.—The facts in regard to the discovery of helium are not less interesting. In 1868 Frankland and Lockyer had observed a particular line D in the solar spectrum which they attributed to the presence of an element—helium—then unknown upon the earth. It was also recognised in the spectrum of Orion and other fixed stars. Subsequently the line of helium was seen by Palmieri (1882) in the lava of Vesuvius, and Hildebrand observed in 1891 what were probably its lines in a spectrum of the nitrogen gas which he got by heating or otherwise treating

uranium ore. While demonstrating argon in the nitrogen gas obtained from Cleveite, Prof. Ramsay observed in 1895 another bright yellow line, and this Sir William Crookes recognized as the D line of helium.

Helium has now been found in many ores, in mineral waters, and in very minute quantities in the air. It is the lightest of all the gases except hydrogen, and Dr. Johnstone Stoney has suggested that this may explain the paucity of these two elements in a free state upon the earth while they are abundant in the universe. As Winkler puts it, "the comparatively small force of the earth's gravitation does not form a sufficient counterpoise to the velocity of their molecules, which therefore escape from the terrestrial atmosphere unless restrained by chemical combination. They then proceed to reunite around great centres of attraction, such as fixed stars, in whose atmospheres these elements exist in large quantities." *

Helium, like argon, is believed to be monatomic, and it is not known to enter into chemical combination. There remains much uncertainty in regard to its position, some maintaining, for instance, that it is composed of two gases.

SUMMARY.—*It is the business of chemistry to distinguish the different kinds of matter, and to study their transformations. Heterogeneous mixtures have to be distinguished from homogeneous compounds and elements. A homogeneous substance which cannot be decomposed by known means is called an element. Careful searching and more ac-*

* Trans. of a paper in *Rep. Smithsonian Inst.* for 1897, p. 244.

curate methods have resulted in an enormous increase in the list of elements in the course of the nineteenth century. Special interest is attached to the recent discovery of argon and helium.

THEORY OF COMBUSTION AND THE CONSERVATION OF MATTER.

Theory of Combustion.—Since the science of chemistry has to do with the changes in the nature of substances when they combine or separate, and since burning is one of the most obvious of these changes, it is natural that we should give prominence to the theory of combustion. But there is another reason why we should do so here, namely, that some understanding of combustion marks the beginning of the century-period with which our brief historical sketch deals. It is hardly too much to say that modern chemistry dates from the time when the burning fire began to be in some measure intelligible, or, what comes almost to the same thing, from the time when, oxygen and carbonic acid gas having been discovered, it became possible to measure the changes which take place in a combustion.

It is interesting, as we sit by the fireside, to think of the different ways in which the familiar sight has been regarded by successive generations of men, from the time when the four elements were first vaguely imagined to the days of "phlogiston" and "principles of combustion," and thence to the present day,—a long story of changing ideas. But it is sufficient for our purpose here to recall, that it was not until about a century ago that there was anything approaching to a scientific vision of the burning fire.

The Greeks and Romans who accepted the four elements of Empedocles—fire, water, earth, and air—regarded fire as a material substance, and combustion as the separation or liberation of the fire-stuff from other material. In the seventeenth century, Becher and Stahl regarded combustion as the separation of "inflammable earth," or the escape of "phlogiston," a compound substance; for "only compound substances can burn." For a long time this Phlogiston theory was generally accepted, and proved a useful stimulus to research. But the repeated demonstration of increase of weight on combustion, the evidence that part of the air is absorbed during the burning, Newton's suggestion that fire was not a special substance at all, and, especially, the discovery of oxygen, hydrogen, carbon-dioxide, and other gases, seriously affected the vitality of the theory, and finally shattered its constitution. It became the subject of most ingenious doctoring, and died a lingering death in the end of the eighteenth century.

What John Mayow, with penetrating insight, had almost discerned more than a century before, that burning means a union of something in the air with inflammable particles in the stuff that burns, became clearer when Priestley discovered oxygen in 1771, when Lavoisier interpreted combustion as oxidation in 1775, and when Cavendish showed that water was a combination of hydrogen and oxygen in 1784.

It is interesting to notice that although Priestley had discovered oxygen and supposed that air supports combustion in virtue of the oxygen which it contains, he died a believer in phlogiston; and that although Scheele—"the ideal of a pure experimental chemist, the discoverer of numberless substances, who

possessed in the highest degree the faculty of observation"—had also discovered oxygen, he was unable to free himself from the bondage of phlogistic theory. The same was true of many others, and it is to Lavoisier (1743–1794) that we must give the credit of destroying the old theory by replacing it with a better. Here we have one of the many instances which lead us to say with confidence that to destroy effectively one must replace. It is true that Lavoisier stood on the shoulders of other workers, but his own experiments were not less ingenious, and, more than any of his predecessors or contemporaries, he reached the importance of precise quantitative measurement. Thus he was led to state about 1777 the fundamental conclusion that in the process of combustion, the burning substance unites with oxygen, whereby an acid is usually produced; and that the increase in weight of the substance burned is equal to the loss in weight of the air. His researches also led him to the general proposition that in all chemical reactions it is only the *kind* of matter that is changed, the *quantity* remaining constant; and to the brilliant idea that "heat is the energy which results from the imperceptible movements of the molecules of a substance."

The Conservation of Matter.—One of the foundation-stones of chemistry—which every worker builds upon with unquestioning confidence—is the conservation of matter. We can neither create nor destroy the smallest particle; the elements which enter into the composition of the soap-bubble film are as lasting as those which form the granite rocks. The state of the matter may wholly change—from solid to gaseous, or in other ways, the particular combinations of the elements may wholly change as they

do when the barrel of gunpowder explodes, but the total amount of matter is the same in the end as it was in the beginning.

The doctrine of the Conservation of Matter states, as Ostwald puts it, that "the total mass of the substances taking part in any chemical process remains constant." And since masses of bodies are at any one place proportional to their weights, the doctrine may read that in any chemical process the weight remains constant. If we change the contents of a sealed vessel by heating, or by mixtures brought about through shaking, or otherwise, we find that the weight at the end equals the weight at the beginning.*

Although the recognition of the conservation of matter was brought about by the work of many, it may be particularly associated with Lavoisier. For one of his earliest investigations, on the supposed conversion of water into earth, he constructed what was at the time the most accurate balance in existence, and he reaped the usual reward of the accurate measurer. When he passed water vapour over red-hot iron turnings and collected the resulting hydrogen, he weighed everything—the water, the iron before and after, and the hydrogen. It was by such typical experiments that "with the balance in his hand, he vindicated the universality of the principle of the conservation of matter." †

The establishment of the general fact of the conservation of matter was of much more than theoretical interest; it was not only a foundation-stone, but a

* W. Ostwald, *Outlines of General Chemistry*, trans. by James Walker, 1890, Chap. I.

† A. Ladenburg, *History of Chemistry*, trans. by L. Dobbin, 1900, p. 21.

touch-stone for chemistry; it supplied a quantitative test by which the accuracy of research could be continually judged.

THE ATOMIC THEORY.

Before Dalton.—The great chemist Berzelius, following his predecessor Richter, quotes on the first page of his classic treatise on *Chemical Proportions* the verse from the Book of Wisdom which says:—

> *Omnia in mensurâ et numero et pondere disposuisti.*
> Thou hast ordered all things in measure and number and weight. —*Sap.* XI. 21.

This may be regarded by some as expressing a remarkable prevision of one of the great results of chemical science,—that exact quantitative relations are always implied in qualitative changes of substance. But whether it was a prevision or not, the verse quoted found no scientific commentary till towards the end of the eighteenth century, and the commentary then begun is still in progress.

The invention of accurate balances—like Lavoisier's—made it possible to pass beyond the detection of chemical elements to some understanding of material architecture. And there seem to have been many who were simultaneously pondering over the problem. Thus Jeremias Benjamin Richter, a mathematical chemist born before his time, published in 1792–1794 a treatise on *Stoicheiometry*, or "the art of measuring chemical elements," in which he showed that acids and bases combine in definite quantitative proportions to form neutral salts. About the same date Proust drew the familiar distinction between chemical mixtures and chemical compounds, pointing out that the latter are characterised by quite definite proportions, whether formed artificially in

the laboratory or found in nature. In 1802 Fischer made the first table of "chemical equivalents," showing what quantities of the different alkaline bases are neutralised by the same quantity of an acid, and conversely for the acids.

But while it is important even in a short historical sketch to observe that scientific discoverers have very rarely a Minerva birth, we must not obscure the fact that though Richter, Proust, and others were working towards a big conclusion, it is to John Dalton that we are indebted for the clear statement of the fundamental fact regarding chemical combination:—that substances, both simple and compound, always combine in definite proportions of their weights. In whatever way one substance is transformed into another, the masses of the two substances always bear a fixed ratio. Even if several substances react together, their masses and those of the new bodies are always in fixed proportions. These facts almost necessarily lead to the atomic conception.

Dalton.—The doctrine of the Quaker chemist depended partly on the following results of experience:—

"No new creation or destruction of matter is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen" (Dalton, after Lavoisier).

In a chemical compound the different constituents are always present in invariable proportions (Dalton, after Proust).

In the interactions of acids and bases, etc., the quantity by weight of an element, or of a compound which takes active part in the chemical change is al-

ways expressible by a fixed number or by a whole multiple of that number. When elements unite with one another in several different proportions—e.g., oxygen and nitrogen—these proportions are related to one another in a simple way. In other words, "If two substances, A and B, form several compounds, of which the compositions are all calculated with respect to the same quantity of A, then the quantities of B combined with this stand to each other in a simple ratio" * (Law of constant equivalents and multiple proportions).

"Thou knowest no man can split an atom" was one of Dalton's sayings, but it should be noted that he meant by an atom the smallest conceivable particle which exhibits the essential properties of the substance in question. Thus he spoke of an atom of water (a compound, $H_2 O$), just as he spoke of an atom of carbon.

With a vision of the *grained structure* of matter clearly before him, he supposed in his theory that while every atom of a given simple substance is like every other atom of that substance, the atoms of different substances have different weights; that in chemical union of elements there is a grouping of definite numbers of elemental atoms into more complex atoms of compounds, and contrariwise in chemical decompositions; and that the elements combine in the proportions indicated by the relative weights of their atoms or in multiples of these. This is the atomic theory "which at once changed chemistry from a qualitative to a quantitative science" (Roscoe).

An examination of some of Dalton's manuscripts has led Roscoe and Harden to the conclusion that

* Ladenburg, p. 55.

he was led to adopt the atomic theory in chemistry in the first instance by purely physical considerations, in opposition to the view generally held that the discovery of combination in multiple proportions led him to invent the atomic theory as an interpretative formula. It seems that Dalton, who was not well aware of contemporary continental work, was led to his great doctrine, not by making an induction from his laborious experiments and measurements, but by a deduction from a theory of the constitution of matter which he devised to account for some of the physical properties of gases. As in many other instances in the development of natural knowledge an important conclusion was reached deductively and then verified inductively.

The way in which Dalton reached his conclusion explains why he gave it the extremely generalised form to which we refer when we speak of the atomic theory. While he was thinking about the definite and fixed quantitative proportions observed in chemical combinations, he was also experimenting with gases (about 1790), and he had visualised these as consisting of distinct particles:—"A vessel full of any pure elastic fluid [that is, gas] presents to the imagination a picture like one full of small shot."

The idea that bodies are formed of distinct particles was not of course Dalton's, but the chemical application was. The idea had been suggested in Newton's *Queries,* and had been used by Boyle, Boerhave, Higgins, and others; it was indeed one of the legacies with which ancient philosophy endowed modern science.

Atomic Weights.—But Dalton was not content to leave the atomic conception in this vague form, he proceeded, in a manner epoch-making though imper-

fect, to determine the relative weights of his hypothetical ultimate particles, and drew up what would now be called a table of atomic weights.

To do this he required a unit of comparison, and he chose hydrogen, the lightest kind of matter known. The weight of an atom of hydrogen was called one. Then, as 8 parts by weight of oxygen combine with 1 part by weight of hydrogen to form water (combining weights), Dalton argued that the atom of oxygen weighed 8 times more than that of hydrogen. And so on for other elements.

It must be borne in mind that the atomic weights were determined with reference to an arbitrary standard, and that they had at first only approximate accuracy.

Summary.—Through the aid of many, but notably through the pioneering genius of Dalton, the atomic theory has won a place among the conceptual formulæ of chemistry. It cannot be said to be proved; indeed, neither "proved" nor "disproved" is an appropriate word to use in regard to these hypotheses. The tests are *convenience, comprehensiveness,* and *consistency* (at once with facts and with other conceptions), and the atomic theory has stood these tests. Forestalling the history a little, we may sum up the general idea in Ostwald's words:

"All substances consist of discrete particles of finite but very small size—of atoms. Undecomposable substances or elements contain atoms of the same nature, form, and mass. If chemical combination takes place between several elements, the atoms of these so arrange themselves that a definite and usually small number of atoms of the combining elements form a compound atom which we call a molecule. Every molecule of a definite chemical

compound (chemical species) contains the same number of elementary atoms arranged in the same way. If the same elements can unite to form different compounds, the elementary atoms composing the molecules of the latter are either present in different numbers, or if their number be the same, they are differently arranged." *

DEVELOPMENT OF THE ATOMIC THEORY.

Dalton's atomic theory, though not final, was fructifying. It prompted a long series of researches which led, after some vicissitudes, to the establishment of the atomic view of nature on a firmer and broader basis. Among the steps of importance, we may especially notice (1) the more accurate determination of atomic weights, (2) the conception of molecules, (3) the kinetic theory of gases, and other physical theories as to the different states of matter, and (4) the development of organic chemistry. The general problem was to form conceptions of material architecture which would harmonise with the facts of chemical change.

Determination of Atomic Weights.—It is well known that each element is conventionally denoted by the first letter or letters of its Latin name, and that with each element a certain number is associated; e.g., 16 with oxygen, 14 with nitrogen, 12 with carbon. This number, or some multiple of it by a whole number, expresses the relative quantity of the given element which enters into compounds. It is the combining mass (or *weight,* though weight must vary with place), or on Dalton's theory, the atomic mass or weight.

* W. Ostwald, *General Chemistry*, trans. 1890.

It has also been noticed that in estimating these numbers, hydrogen is taken as a unit, because it enters into compounds in relatively the smallest weight. The other elements and compounds are tabulated according to the relative amounts of their weights in forming compounds with hydrogen, *or* with some other element whose *equivalent* with hydrogen has been already estimated. When one and the same substance combines in several proportions with another, as nitrogen, for instance, does with oxygen, the smallest number according to which the substance forms combinations is taken, the other numbers relating to the same substance being found to be exact multiples of the smaller. So far the Daltonian rules.

What Dalton began was continued by Berzelius, Turner, and others; but we cannot enter into the record of toil. Only two or three points of interest can be indicated. The process of determining the atomic weight of an element involves: (1) finding the combining proportion or equivalent, and (2) multiplying this by a factor (1—4) decided by the measurement of the vapour density (Avogadro's Law), or by finding the specific heat whose product by the atomic weight is practically constant (Law of Dulong and Petit), or by some other consideration.

Berzelius in his determinations utilised Gay-Lussac's law of volumes (1808) (that two gases always combine in simple proportions by volume), the law of Dulong and Petit (1819), and furthermore the aid furnished by Mitscherlich's discovery of isomorphism (1820). "Mitscherlich established the fact that the corresponding phosphates and arseniates, with the same number of atoms of water, possess the same crystalline form, so that even the secondary

forms coincide. Even at that time, the same number of atoms was assumed to be present in both acids, and thus Mitscherlich arrived at the idea that it was similarity of atomic constitution which gave rise to identity of form." *

This discovery was utilised by Berzelius in the following rule:—"When one substance is isomorphous with another in which the number of atoms is known, then the number of atoms in both is known, because isomorphism is a mechanical consequence of similarity of atomic construction."

"The chemical edifice which Berzelius erected was a wonderful one, as it stood completed (for inorganic substances) at the end of the third decade of the century. Even if it cannot be said that the fundamental ideas of the system proceed exclusively from himself, and if he is indebted to Lavoisier, Dalton, Davy, and Gay-Lussac for a great deal, still it was he who moulded these ideas and theories into a connected whole, adding also much that was original. His electro-chemical hypothesis no doubt had points of similarity with that of Davy, but, in spite of that, it was essentially different from it. Besides, the first method of atomic weight determination, of moderately general applicability, proceeded from Berzelius; and this method was so extraordinarily serviceable that it rendered possible the fixing of these most important numbers, so that alteration was necessary in only a few cases." †

It is important to notice, however, that about 1840 an error of about 2 per cent. was discovered in the estimate which Berzelius had made of the atomic weight of carbon. This raised suspicions and further

* Ladenburg, 1900, p. 96.
† Ladenburg, 1900, pp. 101–102.

inaccuracies were discovered. A revision became imperative, in which Liebig, Dumas, Stas, and others took part. Different methods of determination were discovered, one method was used to check another, stimulus in the arduous task came at different periods from the vision of supposed or real regularities connecting the different numbers (Prout and Meinecke to Mendelejeff and Meyer), and gradually a well-established, well-criticised system of atomic weights was worked out. To Cannizzaro (1858) in particular credit is due for utilising the specific heat method as a check on the others, and Mendelejeff's periodic law furnished, as will be seen, another valuable corrective.

It is a remarkable historical fact, however, that owing to the relative unreliability of the methods for determining the atomic weights, the conception of the chemical atom fell for a time into general disrepute. "At the end of the fourth decade of the century, we find the atomic theory—the most brilliant theoretical achievement of chemistry—abandoned and discredited by the majority of chemists as a generalisation of too hypothetical a character." It was reserved for organic chemistry to re-vindicate it, and for physical researches, especially on gases, to place it on a yet firmer basis.

Physical Enquiries and the Concept of the Molecule.—It is now necessary to allude to a path of physical investigation which had a most important influence on the atomic theory, especially through Avogadro's Law and the kinetic theory of gases.

In 1662, Boyle had stated, as Mariotte did some years afterwards (1679), that the volume of a gas, at the same temperature, is inversely as the pressure. When the pressure increases, the volume diminishes

in inverse ratio. In 1802, Gay-Lussac, whose work touched almost every department of chemistry with important results, stated what had been foreseen (as he says) by Charles fifteen years earlier, that equal volumes of different gases change their volumes equally with equal rise of temperature. Dalton also had perceived this conclusion (the law of Charles) that all gases expand in the same proportion for the same increase of temperature. It should be noted that both these laws (Boyle's and Charles') are ideal formulæ which only approximately fit the facts.

In 1805, along with Alexander von Humboldt, Gay-Lussac observed that exactly two volumes of hydrogen unite with one volume of oxygen to form water. From this starting-point he went on to show (1808) that similarly simple volumetric relations hold true in regard to all gases which combine chemically with one another, and that the volumes of the gaseous products formed always have a simple relation to the volumes of their components (all being measured, of course, at the same pressure and temperature). "Having concluded from their similar behaviour with regard to changes of pressure and temperature that all gases possess a like molecular constitution, Gay-Lussac deduced from his researches (above referred to) the following important law:—The weights of equal volumes of both simple and compound gases, and therefore their densities, are proportional to their empirically found combining weight, or to rational multiples of the latter." * In other words, if gases, like other bodies, combine according to definite proportions of their weights (Dalton's law); and if gases (under the same pressure and at equal temperatures) combine

* E. von Meyer, *History of Chemistry*, trans. 1891, p. 202.

in definite proportions of their volumes (Gay-Lussac's law); then, since density of a gas means the amount of matter measured by weight in the same volume, it follows that the combining weights of gases bear a simple numerical proportion to their densities.

Avogadro's Law.—Another important and closely related result was expressed in 1811 by the Italian chemist, Amadeo Avogadro (1776–1856). He was impressed by the fact that, when there is chemical interaction between gases, there is observable a very simple relation between the volumes concerned. A pint of oxygen combines with two pints of hydrogen to form two pints of steam. Such a simple fact, combined with others relating to the physical properties of gases, led him to suggest that a given volume of any gas (elementary or compound) contains the same number of *molecules* as the same volume of any other gas measured at the same temperature and pressure. *Equal volumes of gases, equal numbers of molecules* is Avogadro's law,—another foundation-stone of modern chemistry. It should be noted that similar views were stated by Ampère in 1814, but neither he nor Avogadro found contemporary recognition or even attention.

Avogadro distinguished between *molécules intégrantes* and *molécules élémentaires,* or, as would now be said, between molecule and atom. "The physical properties of the gases (especially the similarity in their behaviour towards changes of pressure and of temperature) led Avogadro to assume in equal volumes of all gases the same number of molecules; and the distances of the latter from one another he considers to be so great in proportion to their masses, that they no longer exercise any attraction upon one another. These molecules are not sup-

posed, however, to constitute the ultimate particles of matter, but are assumed to be capable of further subdivision under the influence of chemical forces. According to Avogadro, therefore, substances (elements and compounds alike) are not converted, in passing into the gaseous state, into indivisible particles, but only into *molécules intégrantes*, which in turn are composed of *molécules élémentaires*." * The conception of a molecule is that of the smallest portion of a substance which possesses all the properties of that substance; it represents a higher category than atom; thus the molecule of water is represented by the symbol H_2O, which means, in part, that the smallest particle of water consists of two atoms of hydrogen united with one atom of oxygen.

Avogadro's generalisation has furnished one of the main grounds for determining the atomic weights of the elements; and it went far to reconcile Gay-Lussac's discoveries as to gases with Dalton's atomic theory. We have only space to mention that another ground for the determination of atomic weights was furnished by the researches of Dulong and Petit (1818), who showed the close relation between the specific heats of the elements and their atomic weights, and concluded that the atomic heats of all elements (specific heats multiplied by atomic weights) are practically identical; i.e., that all atoms have the same capacity for heat.

Avogadro's recognition of the proportion between the specific gravity of a gas and its molecular weight was slowly appreciated,† but it has borne much fruit.

* Ladenburg, 1900, pp. 61–62.

† Dr. J. T. Merz notes in regard to this belated recognition that Avogadro's hypothesis (1811) is not mentioned in Whewell's *History*, nor in Kopp's (1843–1847), nor in Poggendorf's *Dictionary* (1863).

By improved methods of determining the specific gravity of gases and vapours, "the all-important knowledge of the relative weights of the atoms and molecules of elements and compounds has been immensely advanced" (E. von Meyer, p. 441). From the study of anomalous vapour-densities, H. de St. Claire Deville discovered in 1857 the fact of "dissassociation" or the gradual decomposition of a compound with rise of temperature,—the starting-point for another series of important investigations.

Though confirmed by similar conclusions (Davy, 1812, Ampère, 1814), Avogadro's hypothesis: "*Equal volumes, equal number of particles*" was not appreciated until the establishment of the kinetic theory of gases (q.v.), and "no substantial *chemical* reasons for its adoption were adduced until the year 1846, when Laurent published his work on the law of even numbers of atoms and the nature of the elements in the free state." *

Further Influence of Physical Researches.—When the century was about half over, the doctrine of fixed and multiple proportions was generally accepted (with some saving clauses for not-solid compounds), but the conception of atoms which lay behind this doctrine was looked at more cautiously. The careless may have believed in the physical existence of these smallest indivisible particles, but this was certainly not the general belief. And even as a symbolism, as an alphabet, as a means of notation, there were many chemists who doubted if the atom-concept was indispensable or even legitimate. Corroboration had to come from an independent source, and it came from the physicists, more especially

* Prof. R. Meldola, Address, Section B, *Rep. Brit. Ass.* for 1895, p. 639.

from the kinetic theory of gases, taken in connection with Avogadro's law.

Kinetic Theory of Gases.—As facts began to accumulate showing a remarkable uniformity in the behaviour of different gases to the same changes of temperature and pressure, the need for some conception of the nature of a gas made itself felt in many minds. The early suggestions of Daniel Bernouilli (1738) and of Waterston, Graham's discovery of the law of diffusion, the work of Herapath, Joule and Krönig, the achievements of Clausius (1857–1862) and Clerk Maxwell (1860–1867), are some of the steps in a long history—the history of the kinetic theory of gases, one of the revolutionising concepts of modern science. According to this theory, a gas consists of innumerable particles moving with high velocity, overflowing into any free space which is available, thus securing that there is the same average number in every unit of volume, impinging on the contained walls, if there are any, and thus causing pressure which must obviously increase with the number of the molecules and the mass and velocity of each. Such is at least a suggestion of the view which gave new life to the atomic theory, and that at a time when it was much in want of support. When it was shown that precise and workable conceptions could be formed of the rectilinear movements of molecules in a gas, when the internal motion of the atoms composing the molecules was shown to be a needful assumption, when the rate of velocity of a particle of hydrogen gas was actually calculated, when the laws of Boyle, Gay-Lussac, and Avogadro were brought into harmony, and so on,—chemistry became, in a more real sense than before, a study of the changes of equilibrium in atoms.

Extension of the Atomic Conception.—Here it must be recalled that while physical enquiries into the constitution of matter [or attempts to form a conception of molecular motion] were mainly concerned with gases, the solid and liquid states were also studied. The solid state, where the mass has a proper volume and a proper form, more or less difficult to change, began gradually to be conceived of as one in which the relations of the molecules are such that mutual displacement is not easy. Enquiries into crystallisation begun by Steno (1669), re-stimulated by the genius of Hauy (1781), continued by many workers (Weiss, Von Lang, etc.), also proved suggestive, notably, for instance, when Mitscherlich (1820) elaborated what Klaproth (1798) had observed that the same substance might have different crystalline forms (e.g., calc spar and arragonite).

Gradually, too, the atomic conception was extended to liquids which differ from gases in occupying a definite volume and from solids in having no proper form and much less internal friction. Especially through enquiries into the phenomena of osmosis and of solution, the theoretical conception of gases was applied to liquids. But this was hardly realised till towards the end of the century; indeed it may be associated with the work of Van't Hoff (1887).

Instead of trying to follow the multitudinous lines of research, we propose to take a single illustration —the liquefaction of gases—which may serve to suggest the unity of the different states of matter.

Liquefaction of Gases.—From the time of Faraday's researches in 1823 to the recent work of Dewar, popular imagination has been impressed by the repeated announcement, that such and such a gas had

yielded to the combined effects of high pressure and low temperature, and had been obtained in liquid or solid form. Andrews, Mendelejeff, Pictet, Cailletet, Wroblewski, Olszewski, and many others have contributed to the striking series of experiments.

By a long series of researches, extending through the century, it has been made clear that all ponderable matter may be thought of as essentially of the same nature, irrespective of what its state—solid, liquid, vaporous, or gaseous—may be. The differences of state are conceived of as due to the way in which the relations of the component particles are affected by the greater or less relative activity of the attractive molecular forces and the dispersive thermal motions. As every one knows, water may occur as a solid, a liquid, a vapour, or a gas (saturated steam above 720.6° C.). "Above 30.92° C. carbonic acid is a true gas; no pressure will then liquefy it; but at 30.92° C. a pressure of 77 atmospheres, and below 30.92° C. progressively smaller pressure will condense it; at and below that temperature (Andrews' Critical Temperature) gaseous carbonic acid is a 'vapour,' condensable by pressure alone." * It may also be procured as a solid. Endless examples might be given, for the idea of necessary permanence of state has now disappeared,—and theoretically no case is more striking than another, though technical difficulties have enhanced the interest of some particular instances.

It was about the beginning of the century that Northmore and others liquefied sulphurous acid gas by pressure, but progressive research on the subject began with the work of Faraday and Davy in 1823. They used the method of "enclosing materials from

* Article "Gas," by Daniell, *Chambers's Encyclopædia.*

which the gas can be generated within a tube strong enough to resist the pressure of the gas as it accumulated," and thus chlorine, muriatic acid, carbonic acid, ammonia and many others were liquefied, especially through the energetic work of Faraday.*

In 1835, Thilorier published an account of an experiment, now familiar to students of chemistry, in which he allowed a jet of liquid carbonic acid to escape into a receiver where the evaporation of part of the liquid produced a temperature so low that the rest was frozen into fine snow. In 1845 Faraday combined the method of low temperatures with that of high pressures in the hope of conquering the so-called permanent gases, such as oxygen, hydrogen, nitrogen. But these, along with nitric oxide, carbon monoxide, and methane, resisted his efforts.

In 1869, Andrews expounded his definition of the "critical point,"—the temperature (30.92° C. for carbonic acid) above which no amount of pressure produces visible liquefaction, but below which liquefaction occurs when the pressure is sufficient. "A vapour is a gas at any temperature below its critical point." This step towards clearness led experimenters to recognise that the reason why oxygen, nitrogen, etc., proved intractable was that sufficient low temperatures (below their critical points) were not available.

In 1875–7, by devices securing lower temperatures, Raoul Pictet and Louis Cailletet succeeded in liquefying oxygen. Carbonic oxide, marsh gas, nitric oxide, and others also yielded to the "Cailletet pump," and only nitrogen and hydrogen remained unsubdued. In 1883, nitrogen was liquefied by two Polish workers, Wroblewski and Olszewski. Finally

* Tilden, *Short History of Chemistry*, p. 240.

in 1898, after years of preparation, Professor Dewar produced liquid hydrogen,—a clear, colourless liquid, about one-sixth the density of liquid marsh gas, or about one-fourteenth the density of liquid water at 0°. As Prof. Tilden remarks: "It was both interesting and gratifying that the final victory which crowned the long series of successful attacks upon the apparently impregnable position of the permanent gases should have been recorded in the laboratory of the Royal Institution, where the first successes in this field were won by Faraday." *

DEVELOPMENT OF ORGANIC CHEMISTRY.

Organic and Inorganic Chemistry.—The distinction between the substances found in plants and animals and those in the not-living world is an old-standing one. Rooted in the belief that the substances composing or formed by living creatures were under the domination of a specific vital force, the distinction was for a time accented by the complexity of most of the substances in question, by the fact that they were often difficult to isolate and very ready to change, and by the absence of a secure method of analysing their composition. Later on, the generalisations reached by the students of inorganic substances did not seem to fit in well with what was known in regard to the organic, and the breach was widened. It was thus to a large extent independently that organic chemistry developed, until it became strong enough to react upon the study of the inorganic with a potent and progressive influence.

"At the beginning of the century, when qual-

* For a brief account of the subject the reader is referred to Chapter IX. of Tilden's *Short History of the Progress of Scientific Chemistry*, London, 1899.

itative analysis had already attained a high degree of accuracy, and even the quantitative method had found excellent exponents in Proust, Klaproth, and Vauquelin, Lavoisier's experiments with alcohol, oil, and wax were the only ones in existence, designed to ascertain the composition of organic compounds; and these, it may easily be understood, were not very accurate." *

Some Factors in the Development of Organic Chemistry.—The development of organic chemistry which has been characteristic of the latter half of the century has been influenced in many ways:—by the elaboration of more perfect methods of determining the composition of organic substances (Gay-Lussac, Liebig, Wöhler, Bunsen, Dumas, and many others); by the clear recognition, which may be associated with the name of Berzelius, that organic compounds could not be separated by any hard and fast line from inorganic compounds, but illustrated similar laws, and might in many cases be profitably regarded as derivations of inorganic compounds; by the fascination of the methods of synthesis which gave the chemist an almost creative power; and by the enormous practical interests involved, in connection, for instance, with coal-tar products, one of the most familiar of the many possible illustrations.

We may pause here for a moment to note the fine instance of gradual discovery which the utilisation of coal-tar affords. "Sixty years ago an obscure German chemist obtained an oily liquid from coal-tar oil, which gave a beautiful tint with calcium chloride; five years later another separated a similar liquid from a derivation of coal-tar oil. Still later, Hofmann, then a student in Liebig's laboratory, in-

* Ladenburg, 1900, p. 112.

vestigated these substances and proved their identity with an oil obtained long before by Zinin from indigo, and applied to them all Zinin's term, Anilin. The substance was curiously interesting, and Hofmann worked out its reactions, discovering that with many materials it gives brilliant colours. The practical application of these discoveries was not long delayed, for Perkin made it in 1856. The usefulness of the dyes led to deeper studies of coal-tar products to which is due the discovery of such substances as antipyrin, phenacetin, ichthyol, and saccharin, which have proved so important in medicine." *

Wöhler's Synthesis of Urea.—As analyses of organic substances accumulated, it became perfectly clear that the stuffs composing and formed by living creatures did not contain any peculiar elements. It was seen that they consisted of compounds of carbon with hydrogen, oxygen, nitrogen, and other elements familiar in the organic world.

Those who thought it important to emphasise the distinctions between the living and the not-living then fell back upon the assertion that it was in the arrangement of the elements that the uniqueness of organic substance lay. It was an architectural not a material distinction, and the architect was Vital Force.

It was in the midst of these opinions that Wöhler in 1828 effected the synthesis of urea—the characteristic waste product of higher animals. Starting with cyanic acid, which he had discovered in 1822, he found that urea was formed upon the evaporation of a solution of its ammonium salt. Without the aid of vital force he had formed from a simpler substance a characteristic organic product. It should

* J. J. Stevenson, *Rep. Smithsonian Inst.* for 1897, p. 330.

indeed be noted that he did not build up urea from its elements, but started with cyanic acid, which would now be classed as an organic compound.

Professor Meldola has called attention * to the historical fact that Henry Hennell deserves a place among the pioneers of chemical synthesis, for in 1826–1828 he effected the synthesis of alcohol from ethylene.

Though neither synthesis was complete, the steps were very important. They indicated the beginning of the end of vital force as a chemical factor, the beginning, too, of a remarkable series of synthetic achievements,—trichloracetic acid (Kolbe), formic acid and alcohol (Berthelot), indigo, grape-sugar, and many more—about 180 in all—all of which have been artificially produced.

Isomerism.—Wöhler's synthesis of urea did not quickly find the recognition it deserved, but it doubtless helped to break down the arbitrary distinction between inorganic and organic chemistry, and to further the progress of the latter, which began to be spoken of as the chemistry of the carbon compounds. But Wöhler was also concerned in other steps hardly less significant.

The first of these steps is indicated by the word isomerism. Even Dalton had called attention to the existence of substances of identical chemical composition, but with different properties, and had suggested that this might be explained by different or multiple arrangement of the constituent atoms. But little notice was taken of this. In 1823 Wöhler discovered the composition of cyanic acid; in the following year Liebig reported the same composition for fulminic acid. These two bodies have the same

* *Rep. Brit. Ass.* for 1895, p. 649.

composition, but are very different in character. In 1825 Faraday showed that butylene has the same composition as ethylene (olefiant gas), though the former has twice the specific gravity of the latter. In 1830 Kestner showed that racemic acid has the same composition as tartaric acid, and hundreds of such cases are now known. These facts at first served to complicate matters; they showed that compounds with widely different properties may contain the same constituents and in the same proportions. Berzelius, in labelling the puzzle with the term isomerism, suggested, as Dumas also did, that the component atoms must "be placed together in different ways" in the various isomers, which were the same in composition and yet different in properties. The suggestion seems an easy one, especially when we note that "one chemical compound, a hydrocarbon containing thirteen atoms of carbon combined with twenty-eight atoms of hydrogen, can be shown to be capable of existing in no less than 802 distinct forms" (Roscoe). Indeed, possible substances have been repeatedly predicted, and afterwards discovered or made. But for forty years from Berzelius and Dumas there has been a succession of attempts to show how we may reasonably conceive of composition being the same while the constitution and resulting properties are different. It seems likely that the solution is to be found in the modern development which is called "Chemistry in Space."

Radicals.—But another step with which Wöhler was associated, along with Liebig, Bunsen, Dumas, and others, was the formulation of the radical theory. It was well known that salts are formed from an acid and a base and can be decomposed into these two constituents. For an understanding of the

salt it is more important to recognise its two constituents than to know the quantitative proportions of its component elements. This may suggest the idea, which has been of enormous importance in organic chemistry, that in the usually complex substances involved there exist *groups of elements* which because of their stability of union, may be said to play the part of an element. Such a group is called a compound radical. To take a concrete case, in their researches on bitter almond oil and the allied compounds, Wöhler and Liebig "showed that we may assume the existence, in these substances, of an oxygenated group which remains unchanged in the majority of the reactions, and therefore behaves like an elementary substance. On this account, they called it the radical of bitter almond oil." *

In 1837, Liebig wrote: "We call cyanogen a radical (1) because it is a non-varying constituent in a series of compounds, (2) because in these latter it can be replaced by other simple substances, and (3) because in its compounds with a simple substance, the latter can be turned out and replaced by equivalents of other simple substances." The idea may seem to the outsider far off and theoretical, but there can be no doubt that the formulation of the radical theory not only introduced new clearness into chemistry, but was most provocative of research, some of the results of which have had no small influence on practical human affairs.

Summary.—*Just as it had been shown (Ampère, 1816) that the salts of ammonia can be conveniently discussed and studied by regarding them as salts of a compound element (NH_4) so Berzelius, Dumas, Wöhler, Bunsen, Liebig and others sought to work*

* Ladenburg, 1900, p. 109.

out the idea that organic compounds might be brought into line with inorganic compounds by supposing that they contained compound radicals, like cyanogen, which behaved like elements. In mineral substances the radicals are simple; in organic substances they are compound.

Substitution.—About 1840, Dumas' idea of "substitution" was added to the conceptual formulæ of the organic chemist. "It was found that one or more atoms in an organic compound, notably of hydrogen, might be replaced by an equal number of atoms of other elements, and that such products of substitution retained similar qualities, and could be mutually converted into each other, the type of the compound remaining the same." *

Dumas showed that chlorine may replace hydrogen, atom for atom, in many organic compounds, and "it may be easily imagined how distasteful such a discovery would be to Berzelius and the school of electro-chemists, involving as it does the idea that a negative element may be exchanged for a positive element, without a fundamental alteration in the chemical character of the resulting compound." †

According to Roscoe, the idea of substitution was the germ of Williamson's researches on etherification and those of Wurtz and Hofmann on the compound ammonias—investigations which lie at the base of the structure of modern chemistry—and had also a profound influence on the development of organic synthesis.

Nuclei and Types.—The older radical theory, influenced by the facts of substitution, gave place to the "type theory" of Laurent and Gerhardt and the

* Merz, *History*, Vol. I., p. 410.
† Tilden, *Short History*, p. 15.

conception of "nuclei." "The radical, as the permanent constituent in organic compounds,—corresponding to the elements in inorganic chemistry,—gave way to the changeable nucleus, which only preserved its *form*; the unchangeable principle was found in the form, the structure or type, instead of in the substance of the simple or composite constituents."

Valency.—Time and ability alike fail us to discuss how the endeavour after systematisation and simplicity was continued by Kekulé (1829–1896), Kolbe (1818–1884), A. W. von Hofmann (1818–1892), Wurtz (1817–1884), and many others. The radical theory was characteristically German, the type theory, French; and now we have to notice a more distinctively British contribution,—the idea of the "atomicity" or "valency" of chemical substances, whether elements or compounds. With this idea the name of Frankland (1852) ought perhaps to be particularly associated.

The conception of "valency," or the capacity of saturation of the atoms, was used with great effect by Kekulé. Almost simultaneously, in 1858, he and Couper suggested that the carbon atom should be considered as quadrivalent; i.e., able to unite with four univalent atoms or radicals (such as can replace one atom of hydrogen), but not with more. Kekulé found in this a key to the constitution of many carbon compounds.

"We have chiefly," Ostwald says, "to thank Kekulé for carrying through this idea. In the theory of valency, which is at the present time the prevalent one, it is assumed that each atom possesses a definite limited capacity for combining with other atoms. This capacity is called the valency,

and the atoms that can combine with one, two, three or four atoms (or equivalent atoms or radicals) are said to be univalent, bivalent, trivalent, or quadrivalent respectively. Thus marsh gas CH_4 illustrates the quadrivalent character of carbon, and water OH_2 the bivalent character of oxygen.

Another development, foretold by Wollaston, but practically beginning about 1858, when Pasteur founded "stereochemistry" and Kekulé stated his theory of chemical structure, attained epoch-making expression in 1875, when Van't Hoff published his work entitled *La Chimie dans l'Espace* *—an attempt to formulate a geometrical conception of the manner in which the hypothetical atoms may be supposed to be placed in space. Along with Le Bel, he formulated what is called the theory of "the asymmetric carbon-atom" † and initiated what may be described as a mechanical theory of valency, which has been further strengthened by the work of Wislicenus (1887), and other masters of the chemist's craft.

SUMMARY.—*The development of organic chemistry on its theoretical side affords a fine instance of the gradual specialisation of an hypothesis as the facts require it. The steps indicated by theories of radicals, types, nuclei, and valencies are steps towards a conception of material architecture which will consist with the facts of chemical change.*

The concept of the atom was in its first form too simple; the study of gases showed the necessity of recognising the molecule; the development of organic chemistry enlarged the concept by the suggestion of radicals and nuclei, equivalents and val-

* J. H. Van't Hoff. *Chemistry in Space*, trans. and ed. by J. E. Marsh, Oxford, 1891.

† One whose four valencies are satisfied by four atoms or radicals of different kinds.

encies; the phenomena of right and left handedness led on to ideas of definite geometrical arrangement within the molecule; in these and other ways the atomic theory in its chemical applications has become more and more specialised. "The present position of structural chemistry may be summed up in the statement that we have gained an enormous insight into the anatomy of molecules, while our knowledge of their physiology is as yet in a rudimentary condition" (Meldola, 1895).

THE PERIODIC LAW.

A General Statement by Mendelejeff.—"Many natural phenomena," Mendelejeff says, "exhibit a dependence of a periodic character. Thus the phenomena of day and night and of the seasons of the year, and vibrations of all kinds, exhibit variations of a periodic character in dependence on time and space. But in ordinary periodic functions one variable varies continuously, while the other increases to a limit, then a period of decrease begins, and having in turn reached its limit, a period of increase again begins. It is otherwise in the periodic function of the elements. Here the mass of the elements does not increase continuously, but abruptly, by steps, as from magnesium to aluminium. So also the valency or atomicity leaps directly from 1 to 2 to 3, etc., without intermediate quantities, and in my opinion it is these properties which are the most important, and it is their periodicity which forms the substance of the periodic law. It expresses *the properties of the real elements,* and not of what may be termed their manifestations usually known to us. The external properties of elements and compounds are in periodic dependence on the atomic weights of the elements only

because these external properties are themselves the result of the properties of the real elements forming the isolated elements or the compound. To explain and express the periodic law is to explain and express the cause of the law of multiple proportions, of the difference of the elements, and the variation of their atomicity, and at the same time to understand what mass and gravitation are. In my opinion this is now premature. But just as, without knowing the cause of gravitation, it is possible to make use of the law of gravity, so for the aims of chemistry it is possible to take advantage of the laws discovered by chemistry without being able to explain their causes. The above-mentioned peculiarity of the laws of chemistry respecting definite compounds and the atomic weights leads one to think that the time has not yet come for their full explanation, and I do not think that it will come before the explanation of such primary laws of nature as the law of gravity." *

The general idea of Mendelejeff's periodic law is that the properties of the elements are periodic functions of their atomic weights, but while this is a simplifying concept it is not in any way an explanation.

The Problem of Chemical Classification.—The desire for orderly grouping is one of the mainsprings of scientific work. Even artificial classifications—like the grouping of flowers according to the number of their stamens—have often justified themselves, though they are apt to outlive their usefulness. It is plain that natural classifications—based on deep-seated resemblances—must economise thought and make our outlook on the world clearer. Therefore

* D. Mendelejeff, *The Principles of Chemistry*, trans. 1897, Vol. II., pp. 20–21, foot-note.

it has often been felt that the boon would be great if we could arrange the different kinds of matter in groups or series corresponding in some measure to the classes, orders, families, etc., in which we arrange plants and animals.

It is therefore hardly necessary to say that Mendelejeff was not the first to be attracted by the possibility of detecting serial relations among the chemical elements. Apart from the speculations of the ancients and of the alchemists, glimpses of a supposed orderly relationship of the various elements seem to have been frequent in the history of chemistry. Particularly noteworthy was the idea of a fundamental substance, "protyle" or "prothyle," often identified with hydrogen, of which the other elements were supposed to be derivatives. Prof. Tilden sums up the idea in the quotation:—

> "All things the world which fill
> Of but one stuff are spun."

More concretely, the hypothesis was hazarded anonymously by Prout (1815) that the atomic weights of the gaseous elements are all whole multiples of hydrogen. And with this view, supported by Meinecke (1817), was involved the suggestion that the various elements might turn out to be derivatives of one primary form of matter, such as hydrogen, or something of which hydrogen was an atomic multiple. It was an evolutionist speculation, but born before its time. It has been buried and resurrected several times throughout the century. Defended in Britain by Thomson, scouted by Berzelius, revived by Dumas, it was once more sent to rest about 1860 by Stas, a Belgian chemist, who did splendidly accurate work, from 1860 onwards, in

confirming the doctrine of the regularity of chemical proportions in all combinations.

Others again, without accepting any protyle-hypothesis, pointed out the existence of serial regularities in the atomic weights of the elements, (Lenssen 1857, Pettenkofer 1850, Döbereiner 1817, and even before the atomic theory, J. B. Richter 1798). Döbereiner pointed out that a number of elements could be arranged in groups of three, or triads; e.g., calcium, strontium, and barium, the members of each triad having analogous properties and displaying a certain regularity in the relations of their atomic weights. This idea of family characteristics was afterwards extended by Dumas.

Most noteworthy, however, was the work of Newlands (1863–4), who showed that when the elements were arranged according to the magnitude of their atomic weights, "similar elements were found at approximately equal distances in the series; counting from any one element, every eighth was in general more similar to the first than the other elements." *

As the eighth element, starting from a given one is a kind of repetition of the first, like the eighth note of an octave in music, he called the regularity "The Law of Octaves." He did not succeed, however, in fully carrying out his idea. In the same year (1864), Dr. Odling also published a suggestive paper on "The Proportional Numbers of the Elements and their Serial Relations."

Independent Discovery by Meyer and Mendelejeff. —We accept the conclusion of expert authorities that in 1869 Lothar Meyer and D. Mendelejeff inde-

* Ostwald, *General Chemistry*, trans. by Walker, 1890, p. 35.

pendently reached the same conclusion:—That the properties of the elements are periodic functions of their atomic weights. "If all the elements be arranged in the order of their atomic weights a periodic repetition of properties is obtained. This is expressed by the law of periodicity; the properties of the elements, as well as the forms and properties of their compounds, are in periodic dependence, or, expressing ourselves algebraically, form a periodic function of the atomic weights of the elements." * "If all the elements are arranged in the order of their atomic weights in a series, their properties will so vary from member to member that after a definite number of elements has been passed either the first or very similar properties will recur." † This was the conclusion which Mendelejeff and Meyer expounded.

Let us state the general idea once more. When the elements are arranged according to the magnitude of their atomic weights, "the elements following one another show apparently no regularity in properties, but after the lapse of a certain *period* the chemical and physical behaviour of the elements now succeeding each other strongly recall that of the previous group, in fact, repeat it. The elements which resembled one another were therefore united into groups or *natural families*, and these in their turn were distinguished from the *periods*, which comprised the elements whose atomic weights lay between those of two successive members of a natural family." ‡

Scientific Justification of the Periodic Law.—It

* Mendelejeff, *Principles of Chemistry*, Vol. II., trans. by Kamensky and Greenaway, 1891, p. 16.

† Ostwald, *General Chemistry*, trans. p. 35.

‡ E. von Meyer, *History of Chemistry*, trans. 1891, p. 347.

may be said in a sentence that the general result of chemical work, since Mendelejeff and Meyer stated the Periodic Law in 1869, has been to show that "almost every well-defined and comparable property of the elements appears as a periodic function of the atomic weights" (Ostwald). The atomic volume shows the periodic variation most clearly (Meyer), the melting point of the elements varies periodically (Carnelley), the same holds true of the specific gravities, the magnetic properties of elements depend on the position occupied in the periodic system (Carnelley), there is also a periodicity in the amount of heat developed in the formation of the chlorides, bromides, and iodides (Laurie); these must serve as illustrations of the *manifold* justification which the theory has received.

The Test of Prophecy.—In regard to vital phenomena where the operative factors are usually complex and numerous, there are few who would be willing to submit their favourite generalisations to the severe test of using them as a basis for prophecy, as the astronomer, for instance, can do with some security. But this severe test Mendelejeff did apply to his periodic law.

In his arrangement of elements into groups and series, Mendelejeff was compelled to leave certain blanks. He asserted that these would be filled up by the discovery of new elements.

"He was able to foretell the atomic weights and other properties of these elements from their position in the system, with the aid of the properties observed in the groups and series, which, like a system of co-ordinates, could be called in to assist. Three such blanks occurred in the first five series, and these he indicated as representing the positions of eka-

boron (at. wt. 44), eka-aluminium (at. wt. 68), and eka-silicon (at. wt. 72). Since that time, these three elements have been discovered, and they have been found to possess, approximately, the properties predicted by Mendelejeff. They are: scandium, discovered by Nilson, with atomic weight 44.1; gallium, discovered by Lecoq de Boisbaudran, with atomic weight 70; and germanium, discovered by Winkler, with atomic weight 72." *

To sum up:

"*The periodic law has not only embraced the mutual relations of the elements and expressed their analogy, but has also to a certain extent subjected to law the doctrine of the types of the compounds formed by the elements; has enabled us to see a regularity in the variation of all chemical and physical properties of elements and compounds, and has rendered it possible to foretell the properties of elements and compounds yet uninvestigated by experimental means; it therefore prepares the ground for the building up of atomic and molecular mechanics.*" †

Inorganic Evolution.—An alluring, but perhaps illusory, idea has occurred to many chemists who have pondered over the relations of the elements to one another,—the idea that chemically analogous elements may be related in a real, i.e., genetic, sense, or that they may be derivatives of a common stock. The historians of chemistry have shown that this is an ancient and frequently recurrent idea. Some of the early Greeks imagined one primeval substance developing into all the different kinds of matter;

* Ladenburg, 1900, p. 313.

† Mendelejeff, *Principles of Chemistry*, Vol. II., trans., p. 34.

Boyle spoke of "one universal matter common to all bodies;" Dalton said, "We do know that any of the bodies denominated elementary are absolutely indecomposable;" Graham suggested as conceivable, "that the various kinds of matter now recognised as different elementary substances may possess one and the same ultimate or atomic molecules existing in different conditions of movement." * Many other examples might be given, and we have already referred to the views of Prout, Meinecke, and Thomas Thomson that there is an ultimate relation between hydrogen and the other elements.

In 1888–9 Sir William Crookes again raised the question whether what are called elements may not be compounds, and whether all may not have arisen, by gradual condensation, from hypothetical primitive material which he called protyle.

Accepting the suggestion that substances now thought to be elements may turn out to be compounds, Lockyer has pictured the possible dissociation of the elements in the fervent heat within the sun's atmosphere. It may be so, but there are no certain facts as yet which alleviate the hypothetical character of these imaginings; and it seems well to emphasise that Mendelejeff has expressly dissociated his periodic law from speculations as to the derivation of the elements from one prime matter.

CO-OPERATION OF CHEMISTRY AND PHYSICS.

No two sciences have entered into a co-operation so close as that which now exists between chemistry and physics. In a way the alliance is almost ancient, for chemistry first became an exact science by adopting

* See Sir Henry Roscoe's Pres. Address, *Rep. Brit. Ass.* for 1887, p. 8.

physical methods of weighing and measuring; the balance, which is as familiar an emblem of chemistry as the crucible, is rather a physical than a chemical instrument. But the recognition that chemical and physical properties are inter-dependent and must be studied together, practically dates from Lavoisier, and it has led to a remarkable series of physico-chemical researches which may be said to form a special department of science. Kopp was one of the early workers; Ostwald is now one of the leaders.

Thermochemistry.—A new chapter in the history of chemistry began with Lavoisier's study of combustion and with the resulting recognition of the indestructibility of matter. But Lavoisier left the dynamics of combustion untouched, and another new chapter dates from 1843, from Joule's measurement of the mechanical equivalent of heat, and the resulting recognition of the conservation of energy.* The phenomena of chemical activity assumed a new aspect when it was clearly realised that chemical changes involve only re-distribution, but in no case any destruction of energy or power. This also implied that chemical energy might be measured in terms of the heat evolved or absorbed.

Let us by means of a quotation from Ostwald gain a clear impression of what the main business of thermochemistry is. "Chemical energy is to us the least known of all the various forms of energy, as we can measure neither it nor any of its factors directly. The only means of obtaining information regarding it is to transform it into another species of energy. It passes most easily and completely into heat, and the branch of science which treats of the measurement of chemical energy in thermal units is

* See the Chapter on the Progress of Physics.

called thermochemistry. Thermochemistry is thus the science of the thermal processes conditioned by chemical processes. The quantities of heat evolved or absorbed measure the decrease or increase of chemical energy, in so far as other energy is not involved in the processes." *

Among the important steps in thermochemistry the following may be noted:

The extension of the law of Dulong and Petit by Neumann and later by Regnault (1839); the experiments of Thomas Andrews (1841) on the heat produced during the combination of acid and bases in aqueous solution; Herman Hess's experimental verification (1840) of the conclusion that "the several amounts of heat evolved during the successive stages of a process are the same in whatever order they follow one another"—a conclusion subsequently reinforced by Berthelot; Julius Thomsen's vast accumulation of data (from 1853 onwards) as to heats of formation and all kinds of chemical change; and Berthelot's equally voluminous researches.

We need not, for our purpose, pursue the history further. It is enough to indicate that the aim of discovering the dynamical laws relating to chemical processes is one which has not been lost sight of. At the same time, we have to note the conclusion of an expert like Tilden, that "notwithstanding the labours of half a century, thermochemistry remains for the most part a mass of experimental results, which still await interpretation."

The doctrine of the conservation of energy is the foundation of chemical dynamics. Every change in the arrangement of particles is accompanied by a

* Ostwald, *Outlines of General Chemistry*, trans. 1890, pp. 208–209.

definite evolution or absorption of heat. The object of thermal chemistry is to measure the energy of chemical changes by thermal methods, and thus to get nearer the fundamental problem of the dynamics of chemical affinity.

Photochemistry.—There are few problems more fascinating and more important than those which are raised when we try to follow the transformations of sunlight. Chemical processes in the sun give rise to radiant energy, which is propagated with great velocity ($3 + 10^{10}$ cm. per second) through space, with the ether for its hypothetical vehicle. When it reaches the earth, part of it passes into the form of heat and thence into many other forms, while part of it acting on green plants resumes the form of chemical energy. The radiant energy of sunlight is utilised by the green leaves to split up the carbonic acid of the atmosphere and to build up the complex substances which furnish food and fuel, not to speak of the most valuable super-necessaries of life.

Nor does the radiant energy affect plants only, it has a subtle influence on many animals, modifying for instance the process of coloration, and above all producing those chemical changes in the retina which are associated with vision. In the volume of this series which deals with Inventions due notice will be taken of photography (Daguerre, 1838), which depends on the chemical reactions produced by light on a sensitive surface. But the retina was the first sensitive surface, and we may therefore say that it was in the consideration of problems primarily physiological and secondarily technical that photochemistry, like thermochemistry, had its beginnings.

We have just mentioned the effect of light upon the human eye, and as an illustration from the other

end of the scale of being we may note the attraction of some micro-organisms to light. Thus Engelmann's *Bacterium photometricum*—rod-like purple microbes —not only crowd in a drop of water under the microscope to the particular spot on which the smallest possible beam of light is focussed, but when a microscopic spectrum is projected on the field "select" the area whose colour is that which is most absorbed by their minute bodies.

One other illustration of the chemical action of light upon living creatures may be given, namely, the destructive effect of light upon many kinds of microbes, both in the air and in culture-solutions. We are accustomed to think of light as life-giving, but it also kills. And the fact is significant and full of practical suggestion that sunlight is the most potent, universal, and economical antagonist of some of our worst enemies. How exactly the light kills the bacteria remains somewhat uncertain, but it is commonly believed that it induces too rapid oxidation, that it makes the minute organisms live so fast that they die.

Photochemical research has been as yet in great part concerned with different modes of measuring the chemical activity of light. One of the most successful methods takes advantage of the fact that light induces a mixture of equal volumes of chlorine and hydrogen to form hydrogen chloride (Draper, 1843; Bunsen and Roscoe, 1857). This led to the establishment of the conclusions that the chemical action is proportional to the light intensity, that equal chemical effects are produced when the products of light intensity and time of exposure are equal, that substances are affected differently by different rays, and so on. How it is that light induces chemical

change we do not know, though hypothetical suggestions have been offered.

Photochemistry or the study of the effects of radiant energy (light) on chemical processes is still incipient; though its results have led to the development of photography, the influence of light on the green leaf remains an unread riddle.

Electrochemistry.—It is a familiar fact that if a rod of zinc and a rod of platinum be immersed in dilute sulphuric acid (which does not attack either of them separately), and if the ends of the two rods projecting out of the liquid be apposed or connected by a metal wire, the zinc is dissolved, the hydrogen of the sulphuric acid accumulates on the platinum, and there has come into existence an electric current —a form of energy—which can be made to do work. The source of this energy is in the chemical process, in the heat evolved by the solution of the zinc. By using heat as the common standard of measurement, we are able to prove that a certain amount of potential chemical energy available at the outset is exactly equivalent to the amount of electrical energy produced plus the heat evolved at the seat of the reaction.

From the study of comparatively simple experiments like that above referred to, always in the light of the doctrine of the conservation of energy, electrochemistry has evolved into an important and elaborate department of science.

Faraday distinguished bodies, e.g., metals, which conduct electrical currents without suffering any material change beyond that of heating, from other bodies, such as salts and aqueous solutions of acids and bases, in which the conducted current induces chemical change. "In such conductors of the second class, or electrolytes, the movement of electricity

takes place so that the metals (or metallic radicals) of the salts and bases, and the hydrogen of the acids, move from the positive part of the current to the negative, while the acid radicals or elements, such as chlorine, bromine, iodine, and also the hydroxyl of bases, move in the opposite direction. These components, or ions, are set free where the electrolyte is in contact with metal conducting the current" (Ostwald, *op. cit.* p. 270). In 1833, Faraday formulated the general conclusion, fundamental to subsequent progress, that equal *quantities* of electricity on passing through different electrolytes require equivalent quantities of the ions for their transport. This may be called the foundation-stone of electrochemistry.

It would be interesting to show how the enquiry into the constitution of electrolytes, which must be such that particles charged positively can move in one direction while those charged negatively move in the other, has led through the ideas of Williamson (1851), Clausius (1857), Arrhenius (1887), Planck (1887), to the theory that solutions of salts and of strong acids and bases contain these substances dissociated into ions, that a solution of potassium chloride contains in great part single potassium and chlorine atoms with enormous electrical charges and with their chemical properties thereby modified. It reads like a romance in the invisible world—far more daring than the biologist has ever ventured with his ids and biophors—and yet it appears to harmonise a large number of observed facts. As Ostwald says, "The assumption that electrolytes contain free ions is not only possible but necessary."

It would be interesting also to show how the electric conductivity of electrolytes was measured (Kohl-

rausch, 1880), or how the velocity of the migration of the ions was calculated, or how equations have been worked out and confirmed (Willard Gibbs, Helmholtz, Jahn), showing the relation between the chemical energy, the electrical energy, and the alteration of the electromotive force (i.e., potential, tension or intensity) with the temperature, such that any one of the three can be calculated if the other two terms are known. But we have said enough to suggest the fruitfulness of the co-operation of chemistry and physics in the department of electro-chemistry, and to suggest how well it will repay the reader to avail himself of the pleasure which is afforded by modern chemistry, as expounded by masters like Ostwald.

THE CIRCULATION OF MATTER.

Transformations in Plants.—We have already alluded to the chemist's power of transforming matter. Out of coal-tar he brings the colours of the rainbow and he makes the rubbish of twenty years ago a source of riches to-day.

But any common green plant is the seat of transformations of matter not less marvellous. The elements of soil, water, and air are by the touch of life lifted into complexity, united into organic compounds, forming part of the capital of a living creature.

We are also aware of what Mr. Grove long since called the correlation of the physical forces, what others speak of as the transformations of energy. We know how the energy of the mill-race may drive a dynamo, and we see the energy again in our electric

lamp. We know that heat, light, and electricity are transformable powers.

But any common green plant is the seat of transformations of energy not less marvellous. The energies of the sunlight—the undulations of the ethereal waves, according to the student of physics—are so used by the plant that complex organic substances, of which starch is the first to become visible, are built up. The kinetic energy of the sunlight is changed in the potential energy of complex chemical substances, such as wood. We use such potential energy to supply power to our life, to stoke our engines, to warm our hearths.

We know of no life which is not life-born, but we know that all the world over, from the red-snow plant of Arctic icebergs to the luxuriant vegetation of the Tropics, from the seaweed on the shore to the Californian Wellingtonias, the simple so-called dead elements of water, earth, and air are being quickened into life, that is to say, are becoming part of the capital of living plants. On these plants animals feed, and the wealth of the plants is recoined to feed muscle and nerve, and what was once the dust of the wayside may become part and parcel of the brain of a Cæsar.

Elements in an Organism.—Let us approach the subject in another way. No one knows the chemical nature of living matter, for we cannot isolate what is genuinely alive from associated not-living substance. Moreover, the moment the expert begins his analysis the living matter is dead, and the secret eludes him. But every one now knows the elements out of which the living body is built up, though no one can tell how these elements are arranged in really living stuff nor how they act as they do when thus ar-

ranged. The elements cannot escape the chemist, although their intricacy of arrangement in many cases does.

If we reduce living plants to ashes, and allow nothing to escape undetected, we find a constant presence of twelve elements, carbon, hydrogen, oxygen, nitrogen, sulphur, phosphorus, chlorine, potassium, sodium, calcium, magnesium, and iron. It may be indeed that all the twelve are not present in some of the very simplest forms of life, where the method of ash-analysis is inapplicable. But for ordinary plants which can be burned, the above statement is true. The twelve elements are always present. Had we space, it would be interesting to take each of these elements in turn, to show in what forms they exist in inorganic nature, to follow them from their absorption by root-suckers to their known combinations in plant, animal, or man, and to show how they eventually come back to the so-called dead-state once more. But since it is better to have one definite impression than a hundred vague ones, let us confine our attention to nitrogen.

Circulation of Nitrogen.—As is well known, free nitrogen forms about four-fifths of the atmosphere, but the great bulk of this takes no part in vital processes. With certain notable exceptions it is only in the form of compounds that nitrogen can be used by living creatures. Therefore, since nitrogenous food is *essential* both to plant and animal, the amount of life upon the earth must depend on the amount of fixed nitrogen available.*

The commonest circle is the following: Nitrogen is obtained by the plant in the form of nitrates, ni-

* Bunge, *Text-book of Physiological and Pathological Chemistry*, trans. 1890, p. 19.

trites, or ammonia; these compounds are used in the elaboration of complex nitrogenous bodies such as proteids. These proteids produced by the plant form the food of animals and become part of their vital capital. As the animals live there is a continual disruption of the complex nitrogenous substances and the formation of less complex nitrogenous waste products. This also takes place in plants, but there is this difference, that while the plant retains its nitrogenous waste, the animal gets rid of it—in the form of urea, uric acid, urates, and the like. These waste products rapidly decompose after they have been excreted, and ammonia is formed—available once more to enter upon the cycle.

If the animal or plant die, the agency of putrefactive bacteria brings about decomposition, and the disruption of the nitrogenous materials yields ammonia, nitrates, and the like, which may be again utilised. The availability of nitrogenous material is not thereby affected. On the other hand, as Bunge forcibly points out,* the burning of wood, the cremation of an animal, the explosion of gunpowder, involve a liberation of nitrogen from its fixed or compound form, and a consequent diminution of the available supplies.

"It would appear, therefore, that there is a continuous degradation of nitrogen to the elementary condition—a very serious matter if the nitrogen so degraded is finally removed from the sphere of action of organised beings. Are there, then, any other agencies at work to restore the balance, and enable this apparently useless gas to return within the arena of physiological activity?" †

* Bunge, *op. cit.*, p. 21.

† F. W. Stoddart, "The Circulation of Nitrogen in Nature," *Proc. Bristol Nat. Soc.*, IX. (1899), pp. 57–74.

In the first place, it has to be borne in mind that by electrical discharges in air nitrogen is united with oxygen to form nitric acid, and in a damp atmosphere the same agency causes nitrogen to combine with water vapour to form nitrite of ammonia (Berthelot). The rain after the thunderstorm brings the products to earth.

In the second place, it is stated by Schönbein that wherever evaporation occurs minute traces of ammonia are formed in the air.

In the third place, the researches of Hellreigel and Willfarth, repeated and confirmed by many, show that leguminous plants can under the influence of partner-micro-organisms, which form root-tubercles, utilise (indirectly) the free nitrogen of the air.

In the fourth place, the circulation of nitrogen and the increase of availability is furthered by other lilliputian agencies; namely, those soil-bacteria which convert ammonia into nitrous acid, or carry the oxidation further to the level of nitric acid.

Foundation of Agricultural Chemistry.—If we wish to associate any particular name with the recognition of the fundamental fact of the circulation of matter, it should be the name of Justus Liebig (1803–1873). Himself a student under Gay-Lussac, he became the master of one of the greatest schools of chemistry, the initiator of chemical laboratories, a pioneer of modern organic chemistry, one of the prompters of chemical physiology, the founder of agricultural chemistry, and the discoverer of many important practical applications.

The circulation of elements, of nitrogen for instance, from the air or the soil into plants and thence into animals, and thence back to the soil or air again, is a fact of great interest, justifying us

in speaking of the circulation of matter,—a fact to be associated with Liebig's industry—as not less important than Harvey's theory of the circulation of the blood. The idea marks a new era.

CHEMICAL AFFINITY.

The Problem of Chemical Changes.—Chemistry has above all to do with changes in the composition of matter, and although in point of time the study of chemical changes was prosecuted, by the alchemist, for instance, long before there was any sound knowledge of material composition, the understanding of the former entirely depends on an understanding of the latter.

One of the early results of the careful study of these chemical changes or reactions was to show that though the number of possible experiments is endless, the number of kinds of experiment is limited. It began to be seen that substances could be arranged in various groups, the members of each group acting in a similar way in similar circumstances. Thus a number of substances, like oil of vitriol (sulphuric acid) and spirits of salt (hydrochloric acid) exhibit similar properties, or similar reactions in similar conditions, and may be ranked together as *acids;* another set of substances, like spirits of hartshorn (ammonia) and slaked lime, are most markedly different from the acids, and may be ranked together as *alkalis;* a third set of substances, like chalk, producible by the reaction of an acid and an alkali, may be ranked together as *salts.* Thus there arose a classification of compounds based on similarity of reaction in similar conditions. It was merely a preliminary step towards order, and it led to many others of greater importance.

When two different substances are brought together it frequently happens that changes occur resulting in the production of a new substance or substances. Thus an acid and an alkali, as noted above, produce a salt. Since the indestructibility of matter was recognised, and since Dalton made the atomic conception current coin, it has been evident that the change occurs through a separation and re-combination of the component particles of the two substances. As Dalton said: "All the changes we can produce consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance." But after the phenomena of change have been observed, the question is bound to arise—why should the atoms separate and re-combine at all? Is the phenomenon comparable to anything else in our experience, or is 'chemical affinity' an irreducible fact? Masses attract one another and we can measure the force; is chemical affinity also measurable and does it bear any analogy to gravitation? There is also attraction due to magnetism and different electrical states; has chemical affinity anything to do with this? Thus arises the inevitable problem of chemical affinity; it is still unsolved, but we may profitably consider for a little some of the suggestions which have been offered.

It is part of the work of chemistry to distinguish the different kinds of matter, and we began this historical sketch by alluding to the search for the elements; but a more important problem is to interpret chemical affinity, or the capacity of the elements to exert chemical action.

Electricity and Chemical Affinity.—In the long history of attempts to interpret the chemical activities of different kinds of matter in their relations to

one another, the importance of electrical phenomena has bulked largely. The discoveries of Galvani (1789) and Volta (1792) on the generation of electricity by the use of two metals were not long in being applied to chemistry. Thus in 1800 Nicholson and Carlisle observed that if an electrical current be passed through water, the result is a decomposition into hydrogen and oxygen,—the two gases, namely, which Cavendish, sixteen years before, had shown (synthetically) to be the constituents of water. In 1803 Berzelius and Hisinger published the results of similar experiments on many different compounds, and showed that hydrogen, metals, alkalis, metals, etc., possess positive electrical energy, while oxygen, acids, etc., separate at the positive pole.

Davy.—Meanwhile Humphry Davy had also turned his attention to similar enquiries; he confirmed the results of Hisinger and Berzelius, and made the theoretical suggestion that hydrogen, alkalis, metals, etc., possess positive electrical energy, while oxygen and the acids are correspondingly negative. As oppositely electrified bodies attract each other, the former substances come off in electrolysis at the negative pole (cathode), and the latter at the positive (anode). From this he went on to the momentous generalisation that chemical affinity is due to difference in electrical condition.

Pursuing his decomposition experiments, Davy turned his attention to the alkalis (potash and soda), and found that small metallic globules, burning with brilliancy in air, were formed at the negative pole, while oxygen was evolved at the other. He rightly concluded that the substances he had discovered were the metals Potassium and Sodium, of which the

alkalis are the oxides. This important step, checked by the French chemists, seems to have led many for a time to a false expectation. "The idea was arrived at that the substances hitherto known were only compounds, and that the aim of chemistry was now to discover the true elements, which it was supposed would resemble potassium and sodium. . . . The galvanic current, at that period an entirely new agent, had accomplished this marvel, and it was itself a marvellous thing. By its aid it had become possible to decompose compounds into their true elements; hence it is not surprising that this agency was regarded as identical with the one which gave rise to combinations; i.e., with affinity." *

Berzelius.—The ingenious suggestions of Davy were soon developed by Berzelius into a consistent theory which was then used as the foundation idea of a chemical system.

He believed, with Davy, that all chemical reactions are produced by electricity, which "thus seems to be the first cause of the activity all around us in nature." But he differed from Davy in his mode of conceiving of the electrical distribution. In his own words, "If the electro-chemical views are accurate, it follows that every chemical combination depends wholly and only upon two opposite forces, namely, the positive and negative electricities, and that every compound must be composed of two parts, united by the effects of their electro-chemical reactions, since there is not any third force. From this it follows that every compound substance, whatever the number of its constituents may be, can be divided into two parts, of which the one is positively and the other is negatively electrical."

* Ladenburg, 1900, p. 67.

But difficulties soon gathered round this electro-chemical theory. Even as early as 1834, Dumas showed, in stating his "substitution" theory, that in many organic compounds the positive element hydrogen may be replaced by the negative element chlorine "without a fundamental alteration in the chemical character of the resulting compound." This was practically a deathblow to the theory of Berzelius.

Faraday.—About 1833, Faraday was led to conclude (*a*) that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes, and (*b*) that the proportions of the bodies or ions evolved by an electrolytic action (the electro-chemical equivalents of the ions) are the same as their ordinary chemical equivalents or combining proportions. And he returned to the theory of Davy, saying that "the forces termed chemical affinity and electricity are one and the same."

Sir Henry Roscoe points out that the great principle of valency was foreshadowed from a physical point of view in Faraday's law of electrolysis. Faraday showed that the number of atoms electrolytically deposited is in the inverse ratio of their valencies; Helmholtz in his Faraday lecture explained this by the fact that "the quantity of electricity with which each atom is associated is directly proportional to its valency."

Ionisation Theory.—It does not seem possible, at present, to be confident in affirming or denying the idea that chemical combination is due to the union of electrically charged atoms; but it is certain that the question is not so simple as it appeared to Davy, Berzelius, and Faraday. To make the matter in any way clear it would be necessary to take account of

many researches, notably, for instance, of those concerning the nature of solutions.

The reader should consult, for instance, the eighth chapter of Professor Tilden's *Short History*, especially with reference to the theory of ionisation suggested by Arrhenius.

While the early electro-chemical ideas of Berzelius have been abandoned, a new path of enquiry, especially marked by the work of Svante Arrhenius, continues to be full of promise. Its first milestone bears the date 1884, *when Arrhenius proved that definite and quantitative relations exist between electrical and chemical properties.*

But to this we must add, as suggestive of one of the most significant steps in modern chemical theory, another quotation from Ostwald. "Research based on a well-defined measure of affinity determinable with numerical exactness only became possible, when, by the development of the electrolytic theory of dissociation, the formula was found from which a constant of a general character and independent of the dilution could be calculated. This constant has a claim to serve as a measure of affinity."

While the nature of chemical affinity remains obscure, a mode of measuring it has been attained. If this step is to be associated with any particular name it should be with Ostwald (1889).

CHAPTER V.

The Progress of Physics.

INTRODUCTORY.

Definition of Physics.—"The properties of matter and energy, of energy and ether, and of ether and matter, are the subjects of investigation in physical science." Thus one of the modern masters, Prof. G. F. Fitzgerald,* defined the scope of the science, whose progress in the nineteenth century will be illustrated or suggested in this chapter.

Although we may note Fitzgerald's statement that physical science is divided from chemistry "by being the study of each kind of matter by itself, while chemistry studies the actions of different kinds of matter upon one another," we must also note his acknowledgment—"of course no real line can be drawn."

The physicist has mainly to do with transformations of *energy*, or, in a word, with motion. Or perhaps it is more accurate to say, with Professor J. J. Poynting: "The range of the physicist's study consists in the visible motions and other sensible changes of matter. The experiences with which he deals are the impressions on his senses, and his aim is to describe in the shortest possible way how his various senses have been, will be, or would be affected." †

Method of Physics.—The physicist looks out upon nature seeking for similarities of action—likenesses

* *Science Progress*, Vol. I., 1894, p. 3.
† Address, Section A, *Rep. Brit. Ass.* for 1899, p. 615.

of motion; he groups these together if they are seen to be really the same; he uses instruments to enable his senses to detect hidden motions, and to measure these with accuracy; he tries to find a short descriptive formula of antecedent and sequence which will fit the facts. The so-called laws of motion are "brief descriptions of observed similarities," as Prof. J. J. Poynting expresses it.* As his formulæ increase in number and precision, he often finds it possible to combine several of them in a more general formulæ, which may be so secure, that is so accurate a description, that it affords a basis for safe prediction.

Aim of Physics.—"To take an old but never worn-out metaphor, the physicist is examining the garment of Nature, learning of how many, or rather of how few, different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come. How many different kinds of thread does Nature use? So far, we have recognised some eight or nine, the number of different forms of energy which we are still obliged to count as distinct. But this distinction we cannot believe to be real. The relations between the different forms of energy and the fixed rate of exchange when one form gives place to another, encourage us to suppose that if we could only sharpen our senses or change our point of view we could effect a still further reduction. We stand in front of Nature's loom as we watch the weaving of the garment; while we follow a particular thread in the pattern it suddenly disappears, and a thread of another colour takes its place. Is this a new thread, or is it merely the old thread turned

* Address, Section A, *Brit. Ass. Report* for 1899, p. 616.

round and presenting a new face to us? We can do little more than guess. We cannot get round to the other side of the pattern, and our minutest watching will not tell us all the working of the loom."* But since we cannot rest with discontinuous descriptions, we construct a hypothetical system as to the constitution of matter and the relation of energy to it,—a system in line with what we do know of visible motions and accelerations, —a system to which we will naturally hold until a more complete knowledge should suggest some improvement of it, or, it might be, demand its rejection.

SUMMARY.—*In the main the problem of the physicist is to describe and formulate the likenesses of motion which are observed in our outlook upon nature.*

THE NEWTONIAN FOUNDATION.

At the beginning of the nineteenth century, chemistry was just steadying itself on the foothold afforded by the doctrine of the indestructibility of matter, but Physics had been on sure ground since the publication of Newton's *Principia* (1687). It seems necessary to admit that the value of the Newtonian foundation was not fully appreciated in the eighteenth century, and that many workers left it and built short-lived independent structures, but for the nineteenth century it does not seem too much to say that all stable progress in Physics has been dominated by Newton's conclusions. "In fact the Newtonian philosophy can be said to have governed at least one entire section of the scientific research of the first half of this period: only in the second half of the period have we succeeded in

* Poynting, Address, Section A, *Rep. Brit. Ass.* for 1899, p. 618.

defining more clearly the direction in which Newton's views require to be extended or modified." *

As to the import of Newton's work, three points may be distinguished.

First, it affords what is probably the most striking instance of the application of scientific method, and part of its influence has been that of an illustrious example. It signalised once for all the contrast between metaphysical contemplation and scientific study.

Secondly, in the so-called law of gravitation, which describes "how every particle of matter in the universe is altering its motion with reference to every other particle," Newton not only enlarged the horizon of physics, but gave the world perhaps its finest illustration of a focalising "thought-economising" formula, whose universality and accuracy seem alike indisputable. Here the science passed beyond observation and description to the recognition of a unifying idea.

Thirdly, in his laws of motion and other principles Newton gave a marvellous—if still imperfect—precision to the concepts—of force, matter, and the like—with which the physicist works. Some would say with Prof. Ernst Mach † that Newton "completed the enunciation of the principles of mechanics," or with Thomson and Tait that "every attempt to supersede them has ended in utter failure"; while others would rather say with Karl Pearson that the progress of two centuries has given good reason for trying to modify and restate the *Leges Motûs*, especially in the direction of purifying them, if it be

* J. T. Merz, *History of European Thought*, I., p. 317.

† *Mechanik in ihrer Entwickelung*, 2d ed., 1889, trans. Chicago, by J. T. McCormack, 1893.

possible, from the metaphysical obscurities which lurk even in their apparent lucidity.* But all will agree that Newton supplied the firm foundation on which, especially during the last hundred years, physical science has gradually grown into a stately edifice.

It is doubtless true that Newton stood on the shoulders of Galilei, but his genius in discerning the unity amid multiplicity was none the less great, and there is no finer instance of a unifying idea than the gravitation-formula. At the same time, it must be recognised that, like other big scientific generalisations, the gravitation-theory raised problems which it did not answer.

What we have is a general formula: that every particle or atom or body in the universe attracts every other with a force proportional to their masses taken conjointly, and inversely proportional to the square of their distances apart. This may be called the law of gravitation, but is there no theory of the law? In this respect there has been little advance since the beginning of the nineteenth century.

It was then that Lesage of Geneva suggested that in addition to the gross particles of tangible or sensible matter, "infinite as these are in number, there is an infinitely greater number of much smaller ones darting about in all directions with enormously great velocities. Lesage showed that, if this were the case, the effects of their impacts upon the grosser particles or atoms of matter would be to make each two of these behave as if they attracted one another with a force following exactly the law of gravity. In fact, when two such particles are placed at a distance from one another, each, as it were, screens the other from

* *Grammar of Science*, Chapter VIII., "The Laws of Motion."

a part of the shower which would otherwise batter upon it. . . It is necessary also to suppose that particles and masses of matter have a cage-like form, so that enormously more corpuscles pass through them than impinge upon them; else the gravitation action between two bodies could not be as the product of their masses." * But this speculation is only a provisional stop-gap.

To the easy-going materialists, if any survive, the ignoramus of one of our leading physicists should give pause:—"Directly we use the term 'weight,' we are confronted with the fact that not yet have we any real clew to that astonishing fact of universal gravitation." †

SUMMARY.—*The foundation of modern physics is in Newton's Principia* (1687) *whose value is more fully appreciated at the end than it was at the beginning of the nineteenth century.*

CONSERVATION OF ENERGY.

The Idea of Energy.—Energy is a convenient term for the power of doing work which is possessed by a material system, or by the ether which modern physics has invented as a hazy background of matter. A stream flowing down a valley illustrates energy of motion, it may turn mill-wheels or bear away bridges; the reservoir on the plateau illustrates energy of position, which intention or accident may at any moment bring into operation. These two types of power are, as every one knows, called kinetic energy and potential energy. Whether the kinetic energy be expressed in visible motion, as of the stream, or invisible mo-

* P. G. Tait, *Recent Advances in Physical Science*, 1876, pp. 299–300.

† Prof. Oliver J. Lodge, "Modern Views of Matter," *Internat. Monthly*, I. (1900), p. 525.

tion, as in the particles of a heated bar of iron; whether the potential energy be expressed in a visible arrangement of bodies, as in the stone resting on the roof-edge, or in invisible arrangements, as in the mutual relations of particles in an explosive; we sum up all the different forms in the one conception of energy or power.

The convenience of this concept "Energy" to sum up groups of sense-impressions is obvious, but it must be borne in mind that in using the term we are simply making an abstraction which proves useful in the rapid discussion of the forms or modes of motion which we see and measure. Clerk Maxwell said in his remarkable little book *Matter and Motion:* "We are acquainted with matter only as that which may have energy communicated to it from other matter, and which may in turn communicate energy to other matter," and again, "Energy, on the other hand, we know only as that which in all natural phenomena is continually passing from one portion of matter to another." But, as Karl Pearson points out, these statements do not carry us far. "The only way in which we can understand matter is through the energy which it transfers. . . . The only way to understand energy is through matter. Matter has been defined in terms of energy, and energy again in terms of matter."

"The activity of the material universe," says Prof. Oliver Lodge, "is due to, or represented by, or displayed in, the continual interchanges of energy from matter to ether and back again, accompanied by its transformation from the kinetic to the potential form and *vice versa*." *

* "Modern Views of Matter," *Internat. Monthly*, I. (1900), p. 500.

Transformations of Energy.—Before methods of measuring the different forms of what we call energy had been elaborated, it was evident that one kind of power was continually being changed into another. Carbon and oxygen have in separation potential energy—the energy of chemical affinity for one another, and this is manifested by the heat which they give off when they unite; the heat may be in great part utilised to convert water into steam; the "expansive force" of the steam lifts the piston; the wheels go round; the energy re-appears partly in the potential form of work done and partly in the heat which results from overcoming friction. The energy of the sunlight enables the plant to build up complex food-stuffs out of simple raw materials; substances of high potential energy thus result; these become sources of power to man and beast. The energy of chemical separation may be transformed into heat, light, magnetism, electricity, and so on; or heat, light, and electricity may be used to effect chemical separation. Moreover, all the powers we can employ (except in the case of tidal currents) are directly or indirectly traceable to the energy radiated from the sun, or to stores of potential energy in the earth, which again we have to thank the sun for.

Conservation.—These considerations lead us to the doctrine of the conservation of energy, which is one of the foundations of Physics. It is an induction from experience which states that "the total amount of energy in a material system cannot be varied, provided the system neither parts with energy to other bodies nor receives it from them." * There may be degradation or dissipation of energy, as

* Article "Energy," *Chambers's Encyclopædia*, by Dr. W. Peddie.

when heat passes into the air, but destruction of energy is unknown.

Energy is the power of doing work; work is the act of producing a change of configuration in a system in opposition to resistance; and the doctrine of the conservation of energy is thus expressed by Clerk Maxwell: "*The total energy of any material system is a quantity which can neither be increased nor diminished by any action between the parts of the system, though it may be transformed into any of the forms of which energy is susceptible.*"

Dissipation of Energy.—And to this doctrine of conservation there has to be added the corollary, which Sir William Thomson (Lord Kelvin) first focussed into lucidity (1852)—"the principle of dissipation or degradation," which is "simply this, that as every operation going on in nature involves a transformation of energy, and every transformation involves a certain amount of degradation (degraded energy meaning energy less capable of being transformed than before), energy is becoming less and less transformable." *

Foundation of the Doctrine of the Conservation of Energy.—Just as the doctrine of the indestructibility of matter became stable with the perfecting of the balance, so the doctrine of the conservation of energy must be associated with the determination of the mechanical equivalent of heat,—with the experiments of Rumford and Davy leading on to those of Colding and Joule. At the same time, it should be borne in mind that, according to Thomson and Tait, the principle is clearly implied in Newton's scholium to his third law of motion,—that "if the action of an external agent is estimated by the product of its force

* P. G. Tait, *Recent Advances* (1876), pp. 145–6.

into its velocity, and the reaction of the resistance in the same way by the product of the velocity of each part of the system into the resisting force, arising from friction, cohesion, weight, and acceleration, the action and reaction will be equal to one another, whatever be the nature and motion of the system."

We have placed the doctrine of the conservation of energy before the dynamical theory of heat because many discoveries were pointing towards the great conclusion of the transformability and conservation of energy, before Joule's measurement of the mechanical equivalent of heat made the vaguely foreseen conclusion an established doctrine. None the less, however, would we emphasise that the establishment of the general doctrine dates from Joule's success as a measurer of the relation between heat and mechanical work in 1843.

For it was then that one of the greatest scientific steps of the century was made. "Clear and unquestionable experimental proof was given of the fact that there is a definite relation between mechanical work and heat; that so much work always gives rise, under the same conditions, to so much heat, and so much heat to so much mechanical work. Thus originated the mechanical theory of heat, which became the starting point of the modern doctrine of the conservation of energy. Molar motion had appeared to be destroyed by friction. It was proved that no destruction took place, but that an exact equivalent of the energy of the lost molar motion appears as that of the *molecular* motion, or motion of the smallest particles of a body, which constitutes heat. The loss of the masses is the gain of their particles." *

* T. H. Huxley, Essay on "The Progress of Science" (1887), in *Method and Results*, 1894, pp. 85-86.

While we have given the foremost place to Joule in connection with the doctrine of energy, we must also recognise the genius of Helmholtz, as expressed in his work on *Die Erhaltung der Kraft* (the persistence of force), published in 1847, in which he showed that this great conclusion follows from Newton's second interpretation of the third law of motion, if we make the postulate (sufficiently justified experimentally) of the impossibility of "perpetual motion."

SUMMARY.—"*In his determination of the mechanical equivalent of heat, James Prescott Joule gave to the world of science the results of experiments which placed beyond reach of doubt or cavil the greatest and most far-reaching scientific principle of modern times, namely, that of the conservation of energy.*" *

HEAT AS A MODE OF ACTION.

Old Theory of Heat as a Kind of Matter.—The theory that heat is a subtle kind of matter was suggested by some of the Greek philosophers, and it was a dominant theory in the eighteenth century. In the interpretation of combustion defended by Stahl (1660–1734) a burning body was supposed to give off a substance called "phlogiston." Lavoisier included heat in his list of elements.

Seventeenth Century Theories of Heat as a Mode of Motion.—A more remarkable fact, however, is that in the seventeenth century the modern view was, to say the least, clearly hinted at. As Cajori notes in his *History of Physics*, "We are surprised to find that Newton's immediate predecessors had anticipated our modern theory of heat. *Heat a Mode of*

* Sir Henry Roscoe, Pres. Address, *Rep. Brit. Ass.*, 1887, p. 4.

Motion is the title of Tyndall's well-known work (1862), yet Descartes, Amontons, Boyle, Francis Bacon, Hooke, and Newton already looked upon heat as a mode of motion. Of course, in the seventeenth century, this theory rested upon somewhat slender experimental evidence, else the doctrine could hardly have been cast to the winds by the eighteenth century philosophers."

The Fiction of Imponderable Matter.—Even in the eighteenth century, it could not but be noticed, when the habit of weighing began, that a body which had been heated was no heavier than it was before. Therefore a fiction had to be invented,—the well-known fiction of the "imponderables." Heat, or rather "caloric," was a substance, but it was an imponderable substance. The further difficulty that heat may be produced in abundance apart from all fire or combustion,—even by rubbing two pieces of ice together,—and that it may in other cases disappear beyond trace, seems to the modern outlook quite fatal to the material theory of heat, but the difficulty does not appear to have oppressed the natural philosophers of the eighteenth century. It must be recalled that the doctrine of the indestructibility of matter dates from Lavoisier and that it was not fully appreciated till much later. With this and the doctrine of the conservation of energy now clearly before us, the materiality of heat seems like a contradiction in terms, but this is to be wise after the event.

Let us therefore consider how the old Newtonian idea was re-habilitated, how it has come to be an elementary fact in physics that heat depends upon motion of the particles of a body, and is a form of energy, not a kind of matter.

Rumford's Experiments.—The first strong blow

which the caloric theory received was dealt it by Benjamin Thompson, better known as Count Rumford, who published his observations on the boring of cannon at Munich in 1798. Surprised at the amount of heat given off in the operation, he determined to measure this by its effect in raising the temperature of surrounding water. "At the end of two hours and thirty minutes the water actually boiled!" and Count Rumford argued: "It is hardly necessary to add that anything which an *insulated* body, or system of bodies, can continue to furnish *without limitation,* cannot possibly be a *material substance,* and it appears to me to be extremely difficult, if not impossible to form any distinct idea of anything capable of being excited and communicated in the manner in which heat was excited and communicated in these experiments, except it be *motion.*"

The supporters of the idea that heat is a material substance argued that the production of heat by friction or abrasion was due to the fact that the fragmentation of the body diminished its capacity for holding caloric; and if, as Prof. Tait points out, Rumford had seen his way to a satisfactory experiment which would have tested the capacity for heat of the abraded metal and of the metal before abrasion, then the fact that heat is not matter would have been established. But the essential experiment—most readily a chemical one—did not suggest itself, and this is in part the reason why Rumford's experiments published in 1798 were but little noticed until about 1840.

Rumford's argument was on the main line of progress, but his measurement of the heat evolved by friction was rough, and he was unable to make a definite comparison between the energy expended and the work done and the heat dissipated.

Davy's Contribution.—A more delicate experiment was devised in 1799 by Sir Humphry Davy, who arranged a clockwork for rubbing two pieces of ice against one another in the vacuum of an air-pump, and observed that part of the ice was melted, although the temperature of the receiver was kept below the freezing point. From this he concluded somewhat diffidently that friction causes vibration of the particles, which is heat;—a conclusion which he strengthened in 1812 in the statement that "the immediate cause of the phenomenon of heat is motion and the laws of its communication are precisely the same as the laws of the communication of motion." Thomas Young was another of the early supporters of Count Rumford's view.

Work of Carnot.—Meanwhile important progress was made, by Dulong and Petit (1815), Haugergues (1822), and others, on the measurement of temperatures by means of thermometers; by Faraday and others on the liquefaction of gases, and on many other subjects associated with heat: but the next important step in general theory was made by Sadi Carnot (1796–1832), who, in 1824, published his estimate of the amount of work that can be got from a steam-engine, and introduced the fruitful idea of a reversible cycle of operations. But this was hardly known until Sir William Thomson called attention to it in 1848.

"Without this work of Carnot's, the modern theory of energy, and especially the dynamical theory of heat, could never have attained in so few years its now enormous development." *

"The two grand things which Carnot introduced, which were entirely originated by him, and which left

* Prof. P. G. Tait's *Recent Advances* (1876), p. 95.

him in an almost perfect form, were the idea of a *Cycle of Operations* and the further idea of a *Reversible Cycle.* In order to reason upon the working of a heat-engine (suppose it for simplicity a steam-engine) you must imagine a set of operations, such that at the end of the series you bring the steam or water back to the exact state in which you had it at starting. That is what Carnot calls a cycle of operations, and of it Carnot says, then, and only then, i.e., at the conclusion of the cycle, are you entitled to reason upon the relation between the work which you have acquired, and the heat which you have spent in acquiring it." *

"The other grand point with reference to Carnot is this, that he started the notion of a *Reversible Engine,*—reversible not in the ordinary technical sense of working its parts backwards, not in the mere sense of backing, but reversible in the sense that, instead of using heat and getting work from it, you can drive your engine through your cycle the other way round, and by taking in work, pump back heat (as it were) from the condenser to the boiler again—a reversing of the whole process,—not a mere reversing of the direction in which the engine is driving. Now, Carnot introduced that notion, and he showed by perfectly conclusive reasoning that if you can obtain a reversible engine, it is *the perfect engine;* i.e., that it is impossible to get an engine more perfect than a reversible one." †

Although he began with a firm belief in the caloric theory, Carnot ended to all intents and purposes as an adherent to the modern dynamical view, and that he had grasped the principle of conservation is evident from his conclusion: "Motive power is in

* P. G. Tait, *loc. cit.*, p. 97.

† P. G. Tait, *loc. cit.*, p. 98.

quantity invariable in nature; it is, correctly speaking, never either produced or destroyed."

Joule and Colding.—Prof. Tait notes that one small chemical experiment would have enabled Rumford in 1798 to prove that heat is not matter, just as a little more conclusive reasoning would have brought Davy in 1799 securely to the same conclusion,—which he eventually deduced in 1812.

What Séguin and Mayer approached, but, by departing from the scientific method, failed to attain, was achieved by Colding of Copenhagen and Joule of Manchester, "the true modern originators and experimental demonstrators of the conservation of energy in its generality." *

To Joule in particular, for his experiments were more extensive, his measurements more exact, his conclusions more generalised than those of Colding, we owe a difficult proof of what Rumford and Davy had foreseen—the *First Law of Thermodynamics.* In Tait's statement this reads: "When equal quantities of mechanical effect are produced by any means whatever, from purely thermal sources, or lost in pure thermal effects, then equal quantities of heat are put out of existence or are generated; and for every unit of heat measured by the raising of a pound of water 1 degree Fahrenheit in temperature, you have to expend 772 foot-pounds of work." †

SUMMARY.—*The idea that heat is not material but a mode of motion, a form of energy, is older even than Newton's Principia, yet the foundation of the theory may be fairly dated from the experiments of Joule. But many others contributed to the great conclusion, and still more have furthered its development and application.*

* Tait, *op cit.*, p. 567. † Approximately.

KINETIC THEORY OF GASES.

We have had occasion to refer to this important theory in the chapter on Chemistry; it will be enough to recall two or three of the steps in its development.

Diffusion.—Every one is aware of the rapidity with which an escape of coal-gas makes itself felt through a house. Dalton theorised this in his suggestion that a gas consists of particles which are constantly flying about in all directions, spreading as far as they can, and inter-penetrating another gas, or mixture of gases in the case of air, until equilibrium of pressure is attained.

A more precise study of the movements of gaseous particles was subsequently undertaken by Graham, who showed that the relative rates of diffusion of two gases are inversely proportional to the square roots of their densities. Thus hydrogen diffuses four times more quickly than oxygen.

Joule's Calculation of Velocity of Particles.—In 1848 and 1857, Joule took another stride forward in determining the mean translational velocity of the particles, basing his calculations on the conclusion that the pressure of a gas is proportional to the energy of motion of its particles. "Thus it may be shown that the particles of hydrogen at the barometrical pressure of 30 inches, at a temperature of 60°, must move with a velocity of 6225.54 feet per second in order to produce a pressure of 14.714 lbs. on the square inch." In other words, as Sir Henry Roscoe expresses it, a molecular cannonade or hailstorm of particles is maintained against the bounding surface at a rate far exceeding that of a cannon ball.

It seems that the clearness of the Newtonian view of the movements of the heavenly bodies often suggested to chemists and others who thought about atoms and molecules, that these might be bound together in a manner comparable to a planetary system. But the behaviour of gases and the phenomena of heat (so long regarded as a substance) made it necessary to suppose that forces of repulsion as well as attraction existed between particles. Gradually the intrusion of what Merz calls "the astronomical view of nature" to support the incipient "atomic view of matter" was found unavailing. The atomic view passed from its static to its kinetic phase, and we may particularly associate this important step with the names of Joule, Clausius, and Clerk Maxwell.

Although Bernouilli (1738), Herapath, Waterston and many others must find their recognition in learned histories, it was Joule who first gave precise expression to the theory that all particles of gases may be thought of as being in a natural state of rectilinear motion, changed only by their mutual encounters, or by their impinging on containing barriers. It was soon after the half-century (published 1857) that Joule, as we have noted, calculated the velocity of a particle of hydrogen at ordinary atmospheric pressure and temperature. The calculation presupposed the previous discovery by Rumford, Davy, Mayer, and Joule that heat is not a substance but a mode of motion, and the experimental proof by Joule and Thomson (1853) that in a gas allowed to expand without doing work there is a very slight cooling, due to the energy used up in overcoming the attracting forces of cohesion.

The general argument is simply that if heat can

be transformed into the energy of measurable motion of measurably large or molar masses, heat may itself be "the energy of the directly immeasurable movements of molecular (immeasurably small) masses."

Developments.—"By applying calculations similar to those of Joule, but considerably extended by the use of more powerful mathematical methods, such as the methods of the theory of probabilities, Clausius first, and, a little later, but far more profoundly, Clerk Maxwell, and still more recently Boltzmann, have arrived at very valuable results as to the motions of swarms of impinging particles. One of the results arrived at is that in a mass of hydrogen at ordinary temperature and pressure, every particle has on an average 17,700,000,000 collisions per second with other particles; that is to say, 17,700,000,000 times in every second it has its course wholly changed. And yet the particles are moving at a rate of something like 70 miles per minute. So comes this curious problem—given that the direction of motion of a particle is arbitrarily changed 17,700,000,000 times in every second, and that the particle itself is moving 70 miles in a minute, where would it be at the end of a single minute, having started from any given place? . . . The solution obtained is capable of explaining almost everything that we know with reference to the behaviour of gases, and perhaps even of vapours." *

SUMMARY.—*The kinetic theory of gases, the brilliant generalisation which harmonised the numerous facts—specific heat, diffusion, friction, etc.,—known in regard to the behaviour of bodies in a gaseous state, may be regarded as a corollary of the*

* Tait's *Recent Advances*, 1876, pp. 324–5.

dynamical theory of heat. "The fundamental idea that a gas was an assemblage of moving particles had been put forward by D. Bernouilli and by Herepath, and Joule had in 1851 *made a great step in advance by calculating the mean translational velocity of these particles. . . This idea, in the hands of Krönig and Clausius, gave birth to the modern kinetic theory of gases, which has been so splendidly worked out by Clausius and Maxwell, and since then perfected in detail by Boltzmann, O. E. Meyer, Van der Waals, and many others."* *

UNDULATORY THEORY OF LIGHT.

The Emission Theory.—Throughout the eighteenth century the corpuscular or emission theory of light was almost universally accepted by physicists. The theory was that all luminous bodies emit with equal velocities inconceivably minute elastic corpuscles which travel at great speed in straight lines in all directions.

The Modern View.—Nowadays, however, it is the unanimous view of those who are familiar with the facts that light is not a material substance, but a form of energy, or a mode of motion, in fact the result of ethereal waves. When a body gives forth light, we no longer suppose that it emits corpuscles, as a grain of musk does into the air; we believe that it sets agoing undulatory movements in the ether. We believe furthermore that the phenomena of light are essentially of the same nature as those of electromagnetic radiation. The contrast of the theories in the two centuries is characteristic, and it is interesting to enquire how the modern view was developed.

* E. von Meyer, *History of Chemistry*, trans. 1891, p. 414.

While the corpuscular theory served to interpret a number of the phenomena of light, it failed more or less markedly in regard to others—for instance, the reflection which accompanies refraction, the unequal refrangibility of the different colours of the spectrum, double refraction, and so on. The result was that subsidiary hypotheses had to be invented to cover the defects of the main assumption. Eventually it became necessary to discard the main assumption altogether.

Newton's Position.—The central idea of the undulatory theory was suggested by Hooke and others, and was formulated as early as 1678 by Huygens, who interpreted double refraction, but its establishment was due to the work of Thomas Young and Fresnel. Although Descartes had suggested that light is produced by waves excited in the subtle matter which pervades the universe (analogous to but different from the non-atomic ether of to-day), and had also ventured the suggestion that the mechanism of light and that of gravitation are inseparable, and although Hooke had made the important suggestion of substituting for the progressive wave of Descartes a vibrating one, we find Newton weighing the merits of the wave-theory and the emission-theory, finding both unsatisfactory and deliberately refraining from accepting either. Apart from his "theory of fits,"—in which he states that the phenomena of thin plates prove that the luminous ray is put alternately in a certain state or fit of easy reflection and of easy transmission—he abstains from taking up a definite position, though "he shall sometimes, to avoid circumlocution and to represent it conveniently, speak of it (the emission) as if he assumed it and propounded it to be believed." It does not seem to be

historically justifiable to regard Newton as the founder or even upholder of the emission-theory.*

The ray of light, on the emission-theory, was simply the trajectory of a particle in rectilinear motion; the ray of light, as Newton described it, possesses a regular periodic structure, and the period or interval of fits characterises the colour of the ray. This was an important result. It only required a fitter interpretation to transform the luminous ray into a vibratory wave, but for this there was a century to wait, and Dr. Thomas Young, in 1801, had the honour of discovering it.†

The Wave-Theory of Young.—Thomas Young (1773–1829), whose precocious genius, persisting in manhood, remained, as Tyndall says, "hidden from the appreciative intellect of his countrymen," was led from a study of the eye and its optical properties, to an enquiry into the phenomena of thin plates and "interference," and in the course of this he rehabilitated the undulatory theory (1801), published in the *Philosophical Transactions* for 1802.

The theory is, in general terms, that light consists of vibrations in an all-pervading elastic ether, and that the vibrations, unlike those of sound, are in directions at right angles to the direction of propagation. So far as Young went, the theory was, in simple language, that a homogeneous ray of light is analogous to the wave produced by a musical sound, and that the vibrations of light ought to compose or interfere, like those of sound. "But his hypothesis found no favour; his principle of interference led

* A. Cornu, The Rede Lecture: "The Wave Theory of Light: its influence on Modern Physics," *Nature*, July 27, 1899, pp. 292–297.

† From Prof. Cornu's Rede Lecture.

to this singular result that light added to light could, in certain cases, produce darkness, a paradoxical result contradicted by daily experience." *

In spite of Young's step, the emission-theory still held the field, and new facts, such as the phenomenon of polarisation discovered by Malus, lent support to it rather than to its rival.

Fresnel's Experiments.—In 1816, however, a young engineer, Augustin Fresnel (1788–1827), rediscovered the principle of interference, applied mathematical analysis to the vindication of the undulatory theory, and devised the famous two-mirror experiment, by which it was shown that "two rays, issuing from the same source, free from any disturbance, produced when they met, sometimes light, sometimes darkness." Moreover Fresnel showed that "light is propagated in straight lines because the luminous waves are extremely small, while sound is diffused because the lengths of the sonorous waves are relatively very great," and that "the sound wave cannot be polarised because the vibrations are longitudinal, while light can be polarised because the vibrations are transverse, that is to say, perpendicular to the luminous ray." "Henceforth the nature of light is completely established, all the phenomena presented as objections to the undulatory theory are explained with marvellous facility, even down to the smallest details." †

To Fresnel and to Arago, Young "was first indebted for the restitution of his rights," and it is pleasant to notice the entire absence of any discussion as to priority. But the complete acceptance of the undulatory theory was still distant. There followed a

* Cornu, *loc. cit.*, p. 295.
† Quotations from Cornu.

period in which it had still to struggle for existence, when it had to justify itself in application to the phenomena of shadows, double refraction, polarisation, colour, interference, diffraction and so on.

With Young, Fresnel, Arago, and others on the winning side, with Laplace, Biot, and Brewster and others championing the older doctrine, a keen, sometimes painfully bitter, struggle of opinions continued till the century had run more than a quarter of its course.

Joule.—It should not be forgotten that Joule, who contributed so much to the foundation of the dynamical theory of heat and the kinetic theory of gases, and founded the general doctrine of the conservation of energy, also made an important experiment (1843) bearing on the theory of Light. "He compared the heat evolved in the wire conducting a galvanic current, when the wire was ignited by the passage of the current, with that evolved when (with an equal current, suppose) it was kept cool by immersion in water. These experiments showed a small, but unmistakable, diminution of the heat when light also was given out." *

Foucault.—It was not, however, till 1850 that another crucial experiment in favour of the undulatory theory was announced by Foucault (1819–1868). According to the emission-theory the velocity of light should be greater in an optically denser medium; according to the undulatory theory the reverse should be true. By an ingenious and now familiar device, Foucault, the inventor of the gyroscope and the demonstrator of the Earth's rotation by pendulum experiments, gave the death-blow to the Newtonian

* Tait, *Recent Advances*, 1876, p. 64.

theory by proving that the velocity of light in water is *less* than that in air.

Fizeau.—The determination of the velocity of light, which thus became of importance in relation to the general theory, had been previously based, e.g., by Römer and Bradley, on astronomical data, derived from aberration-observations, or from timing the eclipses of Jupiter's satellites when at their greatest and least distances from the Earth, but a direct experimental method was devised by Fizeau (1819–1896). In 1849, in the suburbs of Paris, he arranged a rapidly rotating cog-wheel which intercepted light at regular intervals, and found what speed must be given to the wheel so that it rotated one tooth's breadth while the light travelled to a distant mirror and was reflected back again. Foucault modified this method by observing "the position ultimately assumed by a ray which travels from a source to a rotating mirror, thence to a distant mirror, and thence back to the original mirror, which by this time has been rotated somewhat." * The determination of the velocity of light thus effected by Fizeau and Foucault was revised by Cornu in Paris, by James Young and George Forbes in Britain, but the most accurate determinations are said to be those made by Michelson, Newcomb, and Holcombe, in the United States. A mean result is that light travels *in vacuo* at the rate of 186,772 miles per second, and in air at a velocity less than this in the ratio of 10,000 to 10,003.

As Professor Alfred Cornu points out in his Rede lecture, to which we have already been much indebted in this section, the emission theory was a natural but primitive one, with its germ in the ex-

* Article *Light*, by Dr. Daniell. *Chambers' Encyclopædia.*

perience of throwing a stone or shooting an arrow into "empty space." The undulatory theory is subtler, space is filled with a continuous elastic medium, in which particles—no longer projectiles—were supposed to oscillate in the direction of propagation, like the particles of water in the ripples on a pond. But this conception was insufficient and gave place to Fresnel's idea of waves of transverse vibrations excited in an incompressible continuous medium.

Electro-magnetic Theory of Light.—The necessity of admitting the existence of this medium was made clearer by Faraday, and corroborated by his discovery of induction, and Clerk Maxwell in his footsteps ventured to forecast, on theoretical grounds, that light and electro-magnetic radiation are alike due to rhythmical disturbances in the ether, differing only in their wave-lengths—one of the most unifying ideas in modern science.

Experiments of Hertz.—"But the abstract theories of natural phenomena are nothing without the control of experiment. The theory of Maxwell was submitted to proof, and the success surpassed all expectation. . . . A young German physicist, Heinrich Hertz, prematurely lost to science, starting from the beautiful analysis of oscillatory discharges by Von Helmholtz and Lord Kelvin, so perfectly produced electric and electro-magnetic waves, that these waves possess all the properties of luminous waves; the only distinguishing peculiarity is that their vibrations are less rapid than those of light. It follows that one can reproduce with electric discharges the most delicate experiments of modern optics—reflection, refraction, diffraction, rectilinear, circular, elliptic polarisation, etc." *

* Cornu. *Rede Lecture.* *Loc. cit.*, p. 296.

We owe to Clerk Maxwell, and to Hertz, for experimental corroboration, the image of a plane wave of light as a propagation of an ethereal disturbance, in which there is electric and, at the same time, magnetic intensity, varying as a simple harmonic function of the time. In what may seem to be plainer words, we regard light as an electric phenomenon, and the term electric light as a tautology.

Invisible Light.—From what has been said it may be inferred that light has many forms, and that it is not necessarily visible. Even in sunlight there are components which are not visible to our eyes.

One of the most recent additions (1896) is that of an invisible radiation which Becquerel discovered to be emitted by many fluorescent substances and especially by Uranium salts. The radiation can be polarised and by means of it (as by the Röntgen rays) photographs can be obtained through opaque bodies. Moreover, like the Röntgen rays, the Uranium-radiation causes an electrified body to lose its charge, whether positive or negative.*

SUMMARY.—*By Young and Fresnel, Fizeau and Foucault and by others the emission theory of light was replaced by the undulatory theory. Light was interpreted in terms of ethereal waves, and Clerk Maxwell and Hertz subsequently showed that it was essentially similar to electro-magnetic radiations.*

THEORY OF ELECTRICITY.

Beginnings.—In the last quarter of the eighteenth century, the Italian Galvani—whose name has given our language several new words—had discovered

* See J. J. Thomson. Address Section A, *Rep. Brit. Ass.* for 1896, p. 703.

that electrical changes occurred in the contracting muscle of the frog's leg; in the last year of the same century Volta of Pavia had shown that electricity may be produced by the simple contact of two metals; but, for a time, little resulted from the discoveries of either of these pioneers. Another impulse was necessary before the wheels of progress began to move, and that was afforded in 1819, by Oersted, who brought the known facts of electricity into touch with those of magnetism, and initiated the movement which has made the word *electricity* almost as characteristic of the nineteenth century as the word *evolution.*

Achievements.—Forestalling the rest of this section, we may briefly state that the scientific study of electricity initiated by Oersted and also by Ampère, was profoundly influenced by the experimental genius and scientific temper of Faraday, found mathematical or precise formulation in the work of Thomson (Lord Kelvin), and was developed into a provisional dynamical theory by the extraordinary insight of Clerk Maxwell. It is perhaps not too much to say that what Newton did for gravitational phenomena, was done by Clerk Maxwell for electrical phenomena. The study was raised by him and his collaborateurs from the observational and classificatory level to become an integral part of a unified Natural Philosophy.

Oersted.—Oersted (1777–1851) may be called the founder of the science of electro-magnetism because he succeeded in proving experimentally (1819) what had been previously surmised, for instance from the effect of lightning on compasses,—that electrical and magnetical phenomena are of the same nature. In his famous experiment showing

the disturbance of the magnetic needle by the influence of an adjacent electrical current, he not only made a step of great theoretical import, but pointed forward (as we now recognise) to the invention of the telegraph.

Oersted's experiment suggested the possibility of measuring the strength of an electric current by its effect upon an adjacent magnet, and this led Schweigger in 1820 to his invention of the galvanometer or electrometer, a fundamental instrument in electrical science. As the history of galvanometers alone is a long one, we must be content here to note that after modifications by Nobili and Pouillet and others, the measuring instrument was brought to great perfection by Sir William Thomson (Lord Kelvin).

Oersted observed the influence of a current on a magnet, and that the latter always tends to set itself at right angles to the direction of the current, but a further step was soon taken by Ampère (1775–1836), who showed (1820) that one current influences another, parallel currents in the same direction being attracted, those in opposite directions being repelled by each other. His mathematical theory of these phenomena is still referred to as a masterpiece.

Ohm.—To Ohm (1789–1854) the science was greatly indebted for the precision which he gave to the conceptions of electro-motive force, strength of current, electric resistance and conductivity, and for the law (experimentally established in 1826, mathematically worked out in 1827) which states that the resistance of a conductor can be measured by the ratio of the electro-motive force between its two ends to the current flowing through it. It appears that this empirical generalisation had been reached in 1781 by Cavendish, but practically its recognition

must date from Ohm's work. "Since his day it has been subjected to the severest experimental tests that the scientific mind could imagine, and has stood them all. It is really the basis of our whole system of electrical measurements, and is to electric currents what the law of gravitation is to planetary motions." *

The instrumental measurement of resistance which Ohm initiated was subsequently brought nearer perfection, especially by those concerned in the development of telegraphy. Thus Charles Wheatstone (1802–1875) invented what is known as "Wheatstone's bridge." Here, as in so many other cases, practical requirements led to improvements which stimulated theoretical science and gave it greater possibilities of precision.

Faraday.—The next great name is that of Michael Faraday (1791–1867), who by common consent is ranked as the greatest experimental genius of the nineteenth century as regards electricity and magnetism. Among his numerous achievements three must be specially mentioned.

While Oersted had shown the deflection of the magnetic needle by an electric current, Faraday succeeded in demonstrating the converse, that a magnet reacts upon an electric current. This was the discovery of magneto-electricity (1831), and it led him on to another of no less importance, that of induced currents (1831),—that a wire through which an electric current is passing may induce in another adjacent wire a state similar to its own. With Faraday's discoveries there must also be associated the entirely independent but synchronous work of the

* Prof. C. G. Knott. Article, *Electricity*, *Chambers' Encyclopædia*.

American Joseph Henry (1799–1878), who also detected the influence of magnetism upon electricity and the phenomenon of induction-currents.

Another of Faraday's achievements has already been referred to in the chapter on chemistry,—the discovery of the laws of electrolysis. He showed that the amount of water decomposed or gas set free is strictly proportional to the quantity of electricity passing through, and that equal quantities of electricity decompose equivalent amounts of different electrolytes.

In the third place Faraday thought out a dynamical theory of electricity, which replaced the old two-fluid theory, and has formed the foundation on which Kelvin, Maxwell, Helmholtz, and others have reared an elaborate superstructure. While Coulomb and others had assumed the possibility of "action at a distance," and supposed that electric charges may influence one another without any intervening medium, Faraday's ideas were distinctly opposed to this view, for he supposed that electric attraction and repulsion were propagated by molecular agitations in the particles of the insulating media which he termed "dielectrics." He found reason to believe that inductive influence takes effect along curved lines ("lines of force") and by the action of adjacent particles in the insulating medium. As the intensity of the electric influence between two charged bodies varies with the nature of the "dielectric," he was led, as Cavendish had been, to the recognition of "specific inductive capacity"—a factor of fundamental importance. As Cajori points out, Faraday's theory gave a death-blow both to the old fluid theory and to the assumption of action at a distance.

Maxwell.—What Faraday had expressed in his

symbolism of "lines of force," was re-expressed and further developed in the sterner language of mathematics by James Clerk Maxwell (1831–1879), who was also led to conclude on theoretical grounds that electro-magnetic phenomena and light phenomena are alike due to waves of periodic displacement in the same medium (the hypothetical ether), and are, in fact, identical in nature.

Hertz.—What Clerk Maxwell had theoretically foreseen was experimentally demonstrated by Heinrich Rudolf Hertz (1857–1894), who detected the electromagnetic (electric and magnetic) waves radiating into space from the sparks of a Leyden jar or of a Holtz machine, separated the two components, electric and magnetic, and succeeded in reflecting, refracting, diffracting, and polarising the waves. "The object of these experiments," he says, "was to test the fundamental hypothesis of the Faraday-Maxwell theory, and the result of the experiments is to confirm the fundamental hypotheses of the theory." * As Hertz fully recognised, Professors Oliver Lodge and G. F. Fitzgerald were about the same time within sight of the same discovery of the electro-magnetic waves in air.

In a review of electrical advance in recent years, Mr. Elihu Thomson notes that the work of Hertz demonstrated "the fact that light of all kinds and from all sources is really an electrical phenomenon, differing from ordinary alternate-current waves only in the rate of frequency of vibrations. We produce electric waves of about one hundred vibrations per second for alternating current work; and in the waves of red light the rapidity is as

* Quoted by Cajori from Hertz's *Electric Waves*, trans. by Dr. E. Jones, London, 1893.

high as four hundred millions of millions of vibrations per second. Hertz and others used waves of some millions per second, and showed how they could transmit signals to distances without wires; these invisible waves being recognised by suitable receivers. The recently announced Marconi wireless telegraph is much the same thing, with certain improvements in detail." *

"Hardly had the work of Hertz and others who followed in his footsteps been assimilated, before the truly remarkable, not to say astounding, discovery by Professor Röntgen of what he called the X-rays produced a profound impression not only in the scientific world, but upon the general public as well. The interest of the scientist had a different basis from the popular one of disclosure of objects hidden in opaque structures; for he saw in the discovery a new weapon of attack upon the secrets of nature. This weapon has already proved to be so serviceable as to show that his anticipations were not unfounded. The X-rays, which became at once indispensable to surgery, are the results of electrical actions in certain vacuum bulbs; and the discovery is properly an electrical one." †

X and other Rays.—It has long been known that remarkable effects are produced when cathode rays are passed through a highly exhausted vacuum tube. The glass shows bright "phosphorescence," shadows are thrown by opaque bodies, and the rays are deflected by a magnet. Crookes and Goldstein have been prominent investigators of the phenomena.

In 1893, Lenard used a tube with a thin window of aluminium, and found that rays passed through

* *Ann. Rep. Smithsonian Inst.*, 1897, p. 135.
† *Loc. cit.*, p. 138.

this outside the tube, affecting photographic plates and electrified bodies. The rays are also affected by a magnet, and Lenard regarded them as prolongations of the cathode rays.

In 1895, Röntgen found that rays issue from the tube which affect a photographic plate after passing through plates, e.g., of aluminium, opaque to ordinary light, which pass from one substance to another without refraction and with little regular reflection. These are apparently not affected by a magnet. They are also remarkable in the way in which they alter the properties (especially the electrical properties) of the substances through which they pass.

Thus, as Professor J. J. Thomson says,* "we may conveniently divide the rays occurring in or near a vacuum tube traversed by an electric current into three classes; without thereby implying that they are necessarily distinctly different in physical character. We have (1) the cathode rays inside the tube, which are deflected by a magnet; (2) the Lenard rays outside the tube, which are also deflected by a magnet; and (3) the Röntgen rays which are not, as far as is known, deflected by a magnet."

Two views are held as to the cathode rays: (*a*) that "they are particles of gas carrying charges of negative electricity, and moving with great velocities acquired as they travelled through the intense electric field which exists in the neighbourhood of the negative electrode"; or (*b*) that they are waves in the ether.

If the nature of the cathode rays is uncertain, so much the more is that of Röntgen's. They differ from light in the absence of refraction, but that may be interpreted as due to the exceeding smallness

* Address to Section A, *Rep. Brit. Ass.* for 1896, p. 701.

of the wave-length; and the same interpretation may account for the absence of conclusive evidence of polarisation.

SUMMARY.—*Of what is meant by an electric charge, the nineteenth century has left us ignorant, but many laws of electrical phenomena have been discovered, and that electrical radiations are best interpreted in terms of ethereal waves is generally conceded. Indeed it has become a question whether all matter may not be resolvable into aggregates of electric charges of opposite sign. But both as regards theory and as regards practical applications, astounding as the progress of these has been,* the twentieth century is pregnant with possibilities of development.*

THEORIES OF MATTER.

Very early in the history of science the idea arose in the minds of enquirers that matter might consist of an aggregation of invisible particles separated by interspaces. This became a precise scientific hypothesis about a century ago, when Dalton developed his Atomic Theory. During the nineteenth century the hypothesis was in several ways developed as fresh facts came to light.

When we see water becoming vapour and again becoming ice, when we see what is usually a gas liquefied and even solidified, when we watch the crystal of sugar melting away in the teaspoon or a crystal of alum growing in a solution of alum, when we consider that many bodies, like iron, expand when heated and contract again as they cool, when we observe that a gas may diffuse through another or even through a

* A fascinating exposition of modern views will be found in an article by Prof. Oliver Lodge, *International Monthly* I. (1900), pp. 483–530.

solid; our instinctive desire to visualise what may be going on beyond the limits of the visible, naturally leads us to imagine matter as having a "grained structure," as being made up of minute particles separated by minute intervals which change with the state of the substance, with conditions of temperature and pressure.

The general idea is simple; the details of the theory are profoundly difficult. "Imagine matter to consist of a crowd of separate particles with interspaces. Contraction and expansion are then merely a drawing in and a widening out of the crowd. Solution is merely a mingling of two crowds, and evaporation merely a dispersal from the outskirts. The most evident properties of matter are then similar to what may be observed in any public meeting." *

Among the many theories of matter, the following stand out prominently.

Perfectly Hard Atoms.—(1) The idea which was expressed by Democritus and Lucretius, which received some measure of approbation from Newton, was that matter consists of perfectly hard atoms with void spaces between these. Newton used this theory in his interpretation of the propagation of sound.

Centres of Force.—(2) A second view, which is associated with the name of Boscovich, replaces the perfectly hard atom by a centre of repulsive and attractive forces. "According to Boscovich an atom is an indivisible point, having position in space, capable of motion, and possessing mass. . . . It has no parts or dimensions; it is a mere geometrical

* J. J. Poynting. Address Section A, *Rep. Brit. Ass.* for 1899, p. 619.

point without extension in space; it has not the property of impenetrability, for two atoms can, it is supposed, exist at the same point." * A similar view was held by Faraday.

Heterogeneousness.—(3) In his *Recent Advances* (1876, p. 288), Prof. P. G. Tait described "a third notion—that the matter of any body, where it does not possess pores, like those, for instance, of a sponge (which obviously does not occupy the whole of the space which its outline fills), fills space continuously, but with extraordinary heterogeneousness." If the moon were built up of irregular stones and mortar, it would seem homogeneous to us (at a distance of 250,000 miles), so the drop of water (removed as it were to a distance by its minuteness) may only be apparently homogeneous.

Vortex Atoms.—(4) A more fertile theory, suggested in 1867, is that of Lord Kelvin—"that what we call matter may really be only the rotating portions of something which fills the whole of space; that is to say, vortex-motion of an everywhere present fluid." †

The beautiful circular vortex-rings which can be so readily made with tobacco or other smoke in air, and with a little ingenuity in water, have very interesting properties (first mathematically deduced by Helmholtz). Thus a vortex ring cannot be cut; "it simply moves away from or wriggles round the knife, and, in this sense, it is literally an atom." ‡ It moves through the air of the room as if it were an independent solid body; one will pass through another and allow that other to pass through it; and it obviously has an extraordinary power of persistence.

* Glazebrook. *James Clerk Maxwell and Modern Physics*, 1896, p. 108.

† *Recent Advances*, p. 20. ‡ *Recent Advances*, p. 297.

But "a common vortex ring of air or water contains within itself the seeds of its own decease; it is composed of an imperfect fluid, possessing that is to say viscosity, and accordingly its life is short; its peculiar energy being dissipated, its vortex motion declines, and as a ring it perishes. But imagine a ring built of some perfect fluid, of some medium devoid of viscosity, as the ether is; then it may be immortal; it can neither be produced nor annihilated by known means; and it is just this property, combined with other properties of elasticity, rigidity, and the like, that led Lord Kelvin originally to his brilliant and well-known hypothesis." *

Thus if the universe be filled with ether, and if that universal medium be a perfect fluid, "then, if any portions of it have vortex-motion communicated to them, they will remain forever stamped with that vortex-motion; they cannot part with it; it will remain with them as a characteristic forever, or at least until the creative act which produces it shall take it away again. *Thus this property of rotation may be the basis of all that appeals to our senses as matter.*" †

The Atomic View of Nature.—Opinions differ as to the fittest way in which to express the facts known in regard to matter, but even those who believe, for instance, that "all matter is resolvable into an aggregate of electric charges of opposite sign," will admit their acceptance of the atomic view of nature, though all may not agree verbally with Prof. Oliver Lodge when he says "a lump of matter is as surely composed of atoms as a house is built of bricks."

* Prof. Oliver Lodge. *Modern Views of Matter.* The International Monthly, I. (1900), p. 501.

† Prof. Tait's *Recent Advances*, 1876, p. 294.

"That is to say," he continues, "matter is not continuous and homogeneous, but is discontinuous; being composed of material particles, whatever they are, and non-material spaces. There is every reason to be certain that these spaces are full of a connecting medium, full of ether; there is no really void space."

But while the atomic view is generally accepted, there is less unanimity as to the fittest conception of the atom. "No one now believes that an atom is simply a vortex ring of ether, and that the rest of the ether is stagnant fluid in which the vortex rings sail about. Any quantity of difficulties surround such an hypothesis as that. Its apparently attractive simplicity is superficial. Nevertheless it is not to be supposed that every hydro-dynamical theory of the universe is thereby denied. It is quite conceivable that a single kind of fluid in different kinds of motion—some kinds of motion not yet imagined perhaps—may possibly be found capable of explaining all the facts of physics and chemistry." *

"I hold," says Prof. Lodge, "that the ether is most certainly not atomic, not discontinuous; it is an absolutely continuous medium, without breaks or gaps or spaces of any kind in it,—the universal connector,—permeating not only the rest of space, but permeating also the space occupied by the atoms themselves. The atom is something superposed upon, not substituted for, the ether, it is most likely a definite modification of the ether, an individualisation, with a permanent existence and faculty of locomotion, which the ether alone does not possess. Matter is that which is susceptible of motion. Ether is that which

* *Modern Views of Matter*, **International Monthly, I. (1900), pp. 499 and 501.**

is susceptible of strain. All energy appertains either to matter or to ether, and is continually passing from one to the other." *

It is now time to turn to the actual progress of scientific discovery and to note a few of the steps which have led towards the modern views of matter, as above suggested.

A. *In Connection with the Kinetic Theory of Gases.*—In his *Hydrodynamica* (1738), Daniel Bernouilli supposed a gas to consist of moving particles, and argued that the pressure, if due to the impacts of these, must be proportional to the square of their velocity.

In 1816 (published 1821), Herapath followed on the same tack, and in spite of fundamental errors (e.g., that the temperature of a gas is measured by the momentum of each of its particles), gave a theoretical justification of Boyle's law (that with constant temperature the product of pressure and volume is constant).

In 1846, Waterston (whose work was overlooked until disinterred from the archives of the Royal Society of London by Lord Rayleigh in 1892) showed that the temperature of a gas "is measured by the mean kinetic energy of a single molecule, and that in a mixture of gases the mean kinetic energy of each molecule is the same for each gas," † thereby furnishing the theoretical basis for the laws of Boyle, Gay-Lussac, and Avogadro.

In 1848, Joule used Herapath's results as a basis for calculating the mean velocity of the molecules of a gas, and obtained from hydrogen at freezing point and atmospheric pressure the value of 6,055 feet

* *Loc. cit.*, pp. 499-500.

† Glazebrook. *James Clerk Maxwell*, 1896, pp. 118-19.

per second, or about six times the velocity of sound in air.

In 1857, in his famous paper "On the Kind of Motion we call Heat," and in his second paper in 1859, Clausius greatly advanced the incipient kinetic theory, calculating, for instance, the average length of the path of a molecule in the interval between two "collisions," or near approaches to another molecule.

In 1859 and 1860, Clerk Maxwell gave his "Illustrations of the Dynamical Theory of Gases" in which he demonstrated "the laws of motion of an indefinite number of small, hard, and perfectly elastic spheres acting on one another only during impact."

By the application of an ingenious statistical method and of general dynamical methods to molecular problems, Maxwell greatly advanced the theory of gases and the theory of matter. That he was helped by Boltzmann and Clausius and Kelvin and others goes without saying, but it seems legitimate to associate with his name the coming of age of the molecular theory of matter. It matters not a whit for our general purpose how many corrections may have to be made on his computation that the length of the mean free path of molecules of air is $\frac{1}{447,000}$ of an inch, or that the number of collisions per second experienced by each molecule is about eight thousand millions; the point is rather that he justified a molecular or atomic conception, harmonising the laws of Boyle, Charles, and Avogadro, and suggesting further developments which are still prompting research.

B. *Cauchy's Suggestion of the Heterogeneity of Matter.*—As a second illustration of the nature of the argument which has resulted in the modern

view or views of matter we may refer to the investigations of the French mathematician, Cauchy, as to the motion of light in solid bodies and liquids. He showed "that if matter were homogeneous, there might be refraction, but there would be no dispersion. All kinds of light would travel with the same velocity in glass, just as they did in the air outside; and, therefore, the mere fact that the different kinds of light can be separated from one another in passing through a prism, gives, at least, a hint that the matter of the prism is heterogeneous, is not infinitely more fine-grained than the length of a wave of any of the kinds of light which it enables us to separate in their courses." * This kind of argument—developed by Lord Kelvin—leads to the result that 400,000,000 in the inch is a rough approximation to the heterogeneity or grained structure of matter.

C. *Other Methods of Estimating the Heterogeneity of Matter.*—In his *Recent Advances in Physical Science* Prof. P. G. Tait gave an account of two other methods ingeniously used by Lord Kelvin in forming an estimate of the grained structure of matter. "The second method was founded upon considerations of the amount of heat which would be generated by electrical action between particles of different materials when they were combined together. The third method was founded upon the forces employed in drawing out a film of liquid,—in fact (to take the simplest case), in blowing a soap-bubble." The various methods yielded approximately the same result, "pointing consistently to something not very largely differing from the 500,000,000th part of an inch as being the distance between the successive particles of matter in a liquid."

* P. G. Tait. *Recent Advances*, 1876, p. 304.

D. *Argument from the Behaviour of Gases.*—Clausius and Maxwell deduced theoretically the conclusion that the length of the mean free path of a moving particle in a gas (i.e., the distance which it will pass through between every two successive collisions), divided by the diameter of any one particle, is equal to the ratio of the whole space occupied by the particles to about eight and a half times the bulk of the whole particles.* In various ways it was found possible to form an equation with approximate data, and the result comes out that the diameter of a particle is not very different from $\frac{1}{250,000,000}$ of an inch.

As a good-sized plum or a small orange is to the whole earth, so is the coarse-grained particle to a drop of water $\frac{1}{8}$ of an inch in diameter.

The calculations of Joule and Clausius, Maxwell and Boltzmann lead to such statements as the following:—"Atoms are big things, the thousand millionth of an inch in diameter, and they cannot travel far without mutual collisions. They are constantly colliding, even in a very good vacuum. In ordinary air every atom strikes another about six thousand million times a second, and it cannot travel even a microscopic distance without collision; its free path is microscopic, or on the average ultra-microscopic." †

E. *From Electrical Phenomena.*—As Prof. Oliver Lodge says, "atoms are big things"—"the thousand millionth of an inch in diameter, and they cannot travel far without mutual collisions." Much too big and cumbrous these are to figure in an interpretation of the cathode rays, the Lenard rays, the Röntgen rays! For here we are brought face to

* See *Recent Advances*, p. 316.

† Oliver Lodge. *Modern Views of Matter.* International Monthly, I. (1900), p. 515.

face with the astounding conception of fragments of atoms, of foundation-stones of atoms, of a unification of all matter in terms of corpuscles of which five hundred or so go to an atom of hydrogen. But the daring speculation carries us further—to doubt whether there is any matter at all, or rather whether inertia is not fundamentally electrical.

Matter and Ether.—We have previously spoken of one of the aims of science as that of finding the common denominator of the fractions of reality which we know. For a time the word Matter was a conspicuous part of this common denominator, but the nineteenth century has left us ignorant of its real nature, and aware only of some of its many properties, and even of many of these properties how little we know. "Impenetrability," the text-books say, and yet Boscovich and Maxwell seem to regard it as conceivable that two atoms should occupy the same space. "Inertia," the text-books say, and yet how little we know of the meaning of this term, how doubtful Lodge seems to be whether there is any but electrical inertia!

The common denominator would now read "the relations of matter, energy, and ether." But the fact is that the scientific conception of matter tends to become more and more monistic. Some years ago we thought of material atoms and molecules, floating in ether, like the crowds of minute organisms in the plankton of the ocean. But various attempts have been made, as Prof. Poynting puts it, "to get rid of the dualism":—Boscovich's theory of point-centres surrounded by an infinitely extending atmosphere of force, Faraday's theory of point-centres with radiating lines of force, Lord Kelvin's theory of atoms as vortices or whirls in a perfect fluid ether,

Larmor's theory of atoms as loci of strain in the ether, and so on. "So, as we watch the weaving of the garment of Nature, we resolve it in imagination into threads of ether spangled over with beads of matter. We look still closer, and the beads of matter vanish; they are mere knots and loops in the threads of ether." *

An Analogy.—An analogy which has often appealed to our biological mind may possibly make the subject clearer. In Biology we are accustomed to speak of three big facts—organism, environment, and function. The environment includes the world of external influences; the organism is the living creature which contains nothing sensible that is not also in the environment; function consists of action and reaction between organism and environment. We do not know the secret of the synthesis which has made it possible for the organism to be a persistent, though ever changeful, a unified and yet reproductive, whirlpool in the stream of the environment. But there it is.

Now it may be that molecules, atoms, corpuscles are persistent unities individualised in the stream or ocean of the ether, as the organism is in its environment, the syntheses being secrets in both cases. And it may be that energy corresponds to function,—consisting of action and reaction between matter and ether.

Summary.—*That matter cannot be conceived as built up of perfectly hard atoms seems quite certain; that it has a heterogeneous structure seems equally certain; some modification of a theory of vortex-atoms would find acceptance as an interpretative*

* J. J. Poynting. Address, Section A, *Rep. Brit. Ass.* for 1899, p. 619.

idea; but it may be that what we call matter will turn out to be conceivable as loops and knots in the threads of ether.

THEORY OF THE ETHER.

Among the concepts which have come to stay in scientific thinking, that of the ether must now be included. It is as real as the concept of "atom" or "molecule," but hardly more so. Perhaps the most natural way of appreciating its validity is by considering some of the facts which have made it seem to many a necessary hypothesis.

Premonition of the Idea.—Long before the nineteenth century, the scientific mind, e.g., Newton's, seemed to feel the need of supposing that there was "something" occupying space between the heavenly bodies.

It does not seem very evident why the extent of distance should make much difference, but, for historical purposes at least, it is well to recall the impression made by the discovery or rather demonstration of the fact that most of the heavenly bodies are at a *literally* immense—unmeasurable—distance from the earth.

Light travels at a rate of about 186,000 miles in a second, and could flash nearly eight times round the earth in that time; but if a hypothetical inhabitant of the nearest star could by any means see the earth, he would see the events of three or four years ago. Now, as we are sure that light is not any kind of stuff or substance, but a form of energy or power, we may, in some measure, understand why to some minds it has seemed necessary to suppose that there is some sort of something linking that star to us.

If light consists of waves, the question naturally arises: "Waves in what?" Especially when the study of polarisation and double refraction showed that the elastic properties of air or water which act as media for sound, will not work when applied to the interpretation of light-phenomena, the conception of the ether forced itself upon physicists.

At first it seems to have been thought of as an exceedingly rare form of matter pervading space and composed of discrete particles; and it was of course invested with the requisite elastic qualities. But gradually the conception became subtler.

Identification of Luminiferous and Electro-magnetic Ether.—The luminiferous ether was invented as a conception which fitted the facts known in regard to light. Similarly Faraday and Clerk Maxwell postulated a special ether for electrical and magnetic phenomena. But when Clerk Maxwell made the further step of showing that one hypothetical medium would suffice for the interpretation of luminous, electric, and magnetic radiations, the case for the ether became much stronger.

That the ether is a necessary conception in modern physics seems to be unanimously admitted by experts, but how exactly the ether is to be conceived of remains quite uncertain.

For some imagine it as an elastic solid, others as a labile fluid, others as a vortex sponge (a phrase which we cannot pretend to explain), and others otherwise.

The modern conception of the ether is that of an absolutely continuous medium, "without breaks or gaps or spaces of any kind in it," "a universal connector," permeating space whether otherwise occupied or not, susceptible of stress, but not of locomo-

tion, probably full of vorticity, but in any case not a stagnant homogeneous fluid, the seat of waves which we call " light " and of others which we call " electromagnetic phenomena," on the whole the most marvellous scientific concept which the mind of man has conceived!

Value of these Hypotheses.—We can well imagine a practical man saying that all this talk of atom and molecule and ether is unreal and unverifiable, and in a certain sense he is undoubtedly right. These molecular and ethereal hypotheses are human imaginings,—and nothing more; they are constructed in terms of one sense, that of sight; they are attempts to see that which is invisible, to invent a machinery of Nature since the real mechanism is beyond our ken; but it must be observed that these hypotheses are not *vain* imaginings, for they prove themselves yearly most effective tools of research, and that they are not *random* guesses, for they are constructed in harmony with known facts.

CHAPTER VI.

ADVANCE OF ASTRONOMY.

FROM COPERNICUS TO NEWTON.

Astronomy an Ancient Science.—Astronomy is usually ranked as the most ancient of the concrete sciences, and this at least is certain that evidence of astronomical observation is furnished by the position of buildings which are much older than all written records. Perhaps one of the first scientific discoveries to become clear and definite was the discovery of the year, with its fine demonstration-lesson of recurrent sequences. From that unknown date to the latest announcements from the observatories of Greenwich and Potsdam, Harvard and Lick, there extends a long procession of discoveries, sometimes almost monotonous in their continuity and sameness, but relieved at intervals by some great and novel achievement which has given a new meaning to the whole.

That astronomy reached a stable position sooner than the other sciences was partly because the sublime subject attracted men of genius who "attended their minds thereunto," and partly because a great part of astronomy is concerned with simple relations of distance, mass, and motion.

Three Main Chapters.—Balfour Stewart has summed up the long history of astronomy in three

main chapters. First it passed through an *observing-period* lasting through thousands of years of nightly study by watchers in the plains of the East to its culmination in the discoveries of Copernicus and Keppler. It then passed into *a stage of analysis and generalisation,* when the genius of Newton rationalised a huge mass of facts in the formula of gravitation. "God said, Let Newton be, and there was light." It finally reached *a stage of deduction,* which, from a knowledge of the positions and movements of the heavenly bodies, predicts their future courses. This might also be called the evolutionary period, since one of its dominant aims has been to show how the solar and other systems have come to be what they are.

The Succession of Systems.—The Ptolemaic system—which placed the earth immovable in the centre of the universe—was superseded by the system of Copernicus (1473–1543), which made the sun the immovable centre. This again was reformed by Keppler (1571–1630), who stated the famous laws or descriptive formulæ of the movements of the planets in their orbits, but was impelled to call in the service of guiding spirits to account for them. Galileo Galilei (1564–1642) was the first to use for systematic study the telescope which the Dutchman, Hans Lippersheim, had invented, and in spite of his revelation of some of the wonders of the heavens—the broken surface of the moon, the countless stars of the Milky Way, the satellites of Jupiter, and the spots on the sun—was almost made a martyr for his dogged adherence to Copernican doctrine. But we must not do more than mention these great names, which are separated by a long interval from the nineteenth century.

The Gravitation Law.—It is necessary, however, to dwell for a little on what is perhaps the greatest of all scientific achievements—Newton's formulation of the Gravitation Law (1687),—the foundation of what has been called the astronomical view of nature. "Every particle of matter in the universe attracts every other particle with a force whose direction is that of the straight line joining the two, and whose magnitude is proportional directly as the product of their masses, and inversely as the square of their mutual distance"—this is the generalisation known as the Law of Gravitation.* Another way of phrasing it may be quoted:—"The law of gravitation states that to each portion of matter we can assign a constant—its mass—such that there is an acceleration towards it of other matter proportional to that mass divided by its distance away. Or all bodies resemble each other in having this acceleration towards each other." † The fundamental concept is that of mutual acceleration.

This formula applies with equal accuracy to a stone falling to the ground and to the motion of the earth round the sun. As far as we know, it is universally true. It may not be possible to trace the logical processes of genius, but it should be noted that just as Keppler deduced his three laws from the observations of Tycho Brahé, so Keppler's laws formed a basis of deduction for Newton.

SUMMARY.—*The science of astronomy, most ancient in its origin, may be said to have passed through three main phases*—(a) *of observation,* (b) *of analysis and generalisation, and* (c) *of deduc-*

* Cited from *Chambers's Encyclopædia.*

† Prof. Poynting, Pres. Address, Section A., *Rep. Brit. Ass.* for 1899, p. 616.

tion; but activity continues on each of these lines, and it may be more accurate to say that the succession of astronomical systems—Ptolemaic, Copernican, Kepplerian, Newtonian, etc., implies mainly a progress in the lucidity, validity, brevity, and universality of descriptive formulæ.

APPLICATIONS OF THE GRAVITATION-FORMULA.

A great part of astronomy is concerned with applications of the gravitation-formula to the phenomena of the heavens; another department has to do with topographical relations, with mapping out positions and orbits; while a third kind of enquiry deals with the physical and chemical nature of the celestial bodies. Laplace, Bradley, and Herschel may be named as representative great masters in these three departments, which have been—not very happily—distinguished as "gravitational," "observational," and "descriptive." Adopting this classification, Mr. Berry notes in his *Short History of Astronomy* * that "gravitational astronomy and exact observational astronomy have made steady progress during the nineteenth century, but neither has been revolutionised, and the advances made have been to a great extent of such a nature as to be barely intelligible, still less interesting, to those who are not experts. . . . Descriptive astronomy, on the other hand, which can be regarded as being almost as much the creation of Herschel as gravitational astronomy is of Newton, has not only been greatly developed on the lines laid down by its founder, but has received—chiefly through the invention of spectrum analysis—extensions into regions not only unthought of, but barely imaginable a century ago."

* P. 355.

In illustrating the century's confirmations and extensions of the gravitational theory, account should be taken of re-estimates of the sun's distance, re-investigations of the movements of the moon and the planets, further elaboration of the theory of the tides, and so on. We have confined ourselves to a brief notice of the discovery of the minor planets, the discovery of Neptune, and the study of comets.

Discovery of the Minor Planets.—Kant had suggested that the zone in which a planet moves might be regarded as the empty area from which its materials had been derived, and that some definite relation should therefore be found between the masses of the planets and the intervals between them. In 1772 Titius pointed out that the distances from the sun of the six planets then known might be represented by a certain numerical series, except that there was nothing to correspond to the term succeeding the one which corresponds to the orbit of Mars. Johann Elert Bode, astronomer in Berlin, filled the gap with a hypothetical planet, and the search for it began. In 1801 Piazzi discovered Ceres, and with the help of Gauss's mathematical genius (used to predict where the planet should be at certain dates) von Zach and Olbers were soon able to confirm Piazzi. In spite of Hegel's protest that the number of planets could not exceed the sacred number seven, a second (Pallas) was soon discovered (1802) by Olbers, and in 1807 four were known. Three of these "asteroids," as Sir W. Herschel called them, corresponded approximately with the requirements of the series indicated by Titius and usually referred to as "Bode's Law," and the idea commended itself that these bodies were the remains of an exploded planet.

As we now know, neither Bode's Law nor the no-

tion of an exploded planet can be regarded as tenable, but both served a useful purpose in prompting research. They led to the recognition of the minor planets, now known to be very numerous (over five hundred) and the discovery must have served as a useful hint of the complexity of relations which further study of the heavens was to reveal. *The story is of interest in illustration of a scientific prophecy which was rewarded even more richly than its basis deserved.*

In 1857 Clerk Maxwell proved the truth of what had been several times suggested—that the rings around Saturn could not be continuous solid bodies nor liquid zones, but that they behaved as if they were composed of a multitude of small solid bodies revolving independently around the planet, somewhat as the minor planets do around the sun. This has received corroboration from telescopic and spectroscopic observations, and is one of the facts which lend countenance to the hypothesis of the meteoric constitution of the heavenly bodies:—that meteoric dust, shooting stars, meteor rings, Saturn's rings, comets, minor planets, nebulæ, and so on, are all, as it were, terms in an evolution-series.

Discovery of Neptune.—There are few chapters in the history of astronomy more familiar, and, at the same time, more instructive, than the story of the discovery of Neptune. It illustrates the method of science,—discovering an anomaly, tracing out the reason for it, and thereby corroborating a general conclusion.

In the first quarter of the century it was repeatedly remarked that the real orbit of Uranus (which Herschel had removed from among the fixed stars to a place among the planets) was not (to the astro-

nomical eye) in harmony with its theoretical path as deduced from the gravitation-formula. To explain these puzzling discrepancies of orbit, it was suggested by several astronomers that they must be due to the influence of some undetected exterior body. But precision was first given to this suggestion in 1845, when John Couch Adams succeeded in calculating out the probable mass and position of the hypothetical planet. In the same year Leverrier (b. 1811) began a similar quest by a different method; in 1846 he determined the probable position of the supposed cause of the disturbance; in the same year he announced that it should be visible in a certain place. He wrote to Galle of the Berlin observatory, told him where to look, and Neptune was discovered. In the same month (September, 1846) the discovery was confirmed by Challis of Cambridge, who had been advised by Adams. It is almost needless to remark on the importance of the discovery as a confirmation of the gravitational formula; here, if anywhere, the exception proved the rule. It should be noted, however, as S. C. Walker first showed, that Neptune had been observed as a fixed star by Lalande in 1795, and furthermore that "the planet was found to have a different orbit from that assigned by the calculators. Their (hypothetical) planets were not identical, nor were they the (real) planet Neptune. But they must ever have credit for the sagacity and ability with which, aiming at so indefinite a target, they so nearly struck the centre." *

The prophetic recognition of the existence of Neptune and its verification may be taken as one of the

* E. B. Kirk. Article, *Astronomy, Chambers's Encyclopædia,* vol. I. p. 528.

finest illustrations of the stability of the gravitational theory.

Comets.—Another series of confirmations of Newtonian laws is concerned with comets. For, although Newton had shown that their movements were in harmony with his general formula, he had few data at his command, and a clearer demonstration was given by Halley, who, from a basis of calculations, accurately predicted the return of "Halley's comet" in 1758–9.

The physician Olbers (d. 1840) introduced a simplification in the method of computing the paths of comets, and for half a century was one of the most assiduous and successful students of these periodic visitants. Among the many whom he helped and stimulated during his long life was Encke, a pupil of Gauss, one of those who have passed through the portal of mathematics to the study of the stars. Sixty-three years after Halley's prediction was verified, Encke in 1822 had a similar success with a comet "of short period," which revolves round the sun in about three and a quarter years.

More than 200 comets have been studied in the nineteenth century; and by means of the spectroscope, applied to the study of comets by Donati in 1864 and by Huggins in 1868, it has been possible to advance a little beyond the computation of paths and periods, and to prove, for instance, that at least some comets are in part self-luminous, while others, especially those of short period, appear to owe most of their brilliance to light reflected from the sun. Professor Tait seems to have been the first to give definite expression to the idea (expounded by Lord Kelvin in 1871) that the light of comets, and of nebulæ as well, may be due to flashes of ignited gas

induced by the encounters amid the swarms of meteoric stones.

It is impressive to read how the comet of 1811 was assigned an orbit requiring 3065 years for its completion, such that "when it last visited our neighbourhood, Achilles may have gazed on its imposing train as he lay on the sands all night bewailing the loss of Patroclus; and when it returns, it will perhaps be to shine upon the ruins of empires and civilisations still deep buried among the secrets of the coming time." It is impressive to note the measurements of some of the great comets whose highly rarefied emanations or "tails" may extend for several millions of miles, but the behaviour of the tail points to the conclusion that it is but "a stream of matter driven off from the comet in some way by the action of the sun," and the density must be small indeed, since the earth has passed through a tail at least twice in the century without the fact being known until afterwards. Indeed the progress of knowledge has robbed comets of some of their dignity, for since the middle of the century it has been generally recognised that, with the possible exception of the bright central "nucleus," a comet is small in mass, and in a state of great tenuity, unable to affect the motion of the planet it approaches, and allowing the light of a star to pass even through its "head."

Numerous interesting observations point to some close connection between comets and meteors or "shooting stars." Thus Biele's comet (with a period of sixty-seven years), which scared the popular imagination in 1832, was first seen to become double, and was afterwards lost altogether, while on two sub-

sequent occasions (1872 and 1885), as the earth was crossing the path of the comet when it (if it had persisted) was nearly due at the same place, there was an unusually brilliant shower of meteors. Meteors may be fragments of a broken-up comet, or a comet may be a swarm of meteors.

In the study of comets the accuracy of the gravitational formula has been beautifully illustrated, and, during the latter half of the century, considerable progress was made towards an understanding of their physical nature.

THE STUDY OF THE STARS.

Almost until the end of the eighteenth century, it was the general belief, even among astronomers, that the stars were fixed and unchanging. As Miss Clerke says, "their recognised function, in fact, was that of milestones on the great celestial highway traversed by the planets." Gradually, however, it became evident that this emphatically static image was far from being true. What Giordano Bruno had imagined, was confirmed by Halley in 1718, when he showed that Sirius, Aldebaran, Betelgeuse, and Arcturus had changed their positions in the sky since Ptolemy marked these out. Many similar facts came to light, and in the last quarter of the eighteenth century, sidereal astronomy included "three items of information—that the stars have motions, real or apparent; that they are immeasurably remote; and that a few shine with a periodically variable light." *

William Herschel.—It was about the beginning of

* Agnes M. Clerke. A popular *History of Astronomy During the Nineteenth Century*, 1885, p. 13.

the last quarter of the eighteenth century that William Herschel (1738–1822) began to realise his ambition of obtaining "a knowledge of the construction of the heavens," and rapidly passed from being "a star-gazing musician" to the post of royal astronomer.

He made clear, what had been suspected by some, that there were systems of stars, in some measure comparable to the planetary system, but varying greatly in the periods and forms of their revolutions. A double star had been *usually* regarded as an optical phenomenon due to the fact that two stars which might be very far apart, happened to be nearly in the same line of sight from the earth; Herschel proved that many double stars were real binary combinations, "intimately held together by the bond of mutual attraction." In the apparent motions of the stars he distinguished one component due to a translation of our planetary system towards a point in the constellation Hercules, and another component due to a real movement of the stars themselves. In his study of nebulæ he was gradually forced to the conclusion that there were nebulosities which could not be resolved in stars, but consisted of a "shining fluid" or "self-luminous matter" diffused in space, and "more fit to produce a star by its condensation, than to depend on the star for its existence." This led him about 1791 to a theory of the nebular origin of stars, apparently in complete independence of the nebular theory of Laplace (1796).

Two main contributions, then, must be traced to Herschel,—that he extended Newtonian methods to the study of the stars, and that he made the whole scientific picture of the heavens vividly kinetic. On the one hand, he extended the range of precise

measurement and calculation; on the other hand, he emphasised the idea of change or, one may almost say, of evolution. The heavens no longer seemed fixed and unchanging, when it was shown that new systems were being formed and that others were dying away.

Herschel's work was continued at Königsberg by Bessel; at the Russian observatory of Pulkowa by Struve, succeeded in 1858 by his son Otto; and by many other illustrious workers. In Britain the father found an intellectual heir in the son, John F. W. Herschel (1792–1871), whose Cape observations (1834–38) did for the Southern heavens what had been done for the Northern. Published in 1847, they represent the state of sidereal astronomy at the middle of the century. "Not only was acquaintance with the individual members of the cosmos vastly extended, but their mutual relations, the laws governing their movements, their distances from the earth, masses, and intrinsic lustre, had *begun to be* successfully investigated." *

Improvements in telescopes and other instruments aided in the progress of the sidereal astronomy to which Herschel had given so much impetus, and with improved mechanical means was associated a reformed method of observation. Friedrich Wilhelm Bessel (1784–1846), who made himself famous at the age of twenty by calculating an orbit for Halley's comet, did a gigantic piece of work by instituting (1813, 1830) a uniform system of "reduction" (or, correction of observations) which lengthened out the period of exact astronomy by half a century. In other words he made a uniform correction of Bradley's Greenwich observations, making allowances for

* A. M. Clerke. *History*, 1885, p. 65.

precession, aberration, refraction, and instrumental errors. And the edition of Bradley's results was only a prelude to fresh catalogues of his own, executed between 1821 and 1833, and including about 62,000 stars. It is hardly necessary to say that Bradley's work was continued through the century by many illustrious astronomers.

Measuring the Distance of a Star.—To the ancients the stars remained altogether mysterious; they were points of fire set in the concave vault of the firmament and borne by it in daily revolution around the fixed earth. Keppler seems to have been the first to dare to deduce from the Copernican system the conclusion that the stars are extremely distant suns,—so distant that most of them appear unaffected in direction throughout the year; e. g., when viewed from opposite ends of the earth's orbit. If so distant and yet so clearly visible, they must be sunlike; i. e., great sources of radiant energy. This conclusion was less hesitatingly accepted by Galilei.

But while it came to be generally recognised that the stars were unthinkably distant suns, it was not till 1838 that the distance of any star was measured. In that year, Friedrich Wilhelm Bessel (1784–1846), using Fraunhofer's heliometer, or "divided object-glass micrometer," was able to determine the parallax, and thus to deduce the distance of a small star in the constellation of the Swan (61 Cygni). Soon afterwards analogous results were published by Thomas Henderson for α Centauri (1839), and by Struve (1840) for Vega.

The method of estimating the distance of a star is simple in theory. Suppose that the direction of a star is observed at a certain time with all possible accuracy; suppose that the same star is observed

six months later when the earth has travelled over one-half of its orbit, another direction-line may be observed; suppose the two direction-lines produced till they meet, the point of intersection must be the position of the star. Then we have a triangle whose base is the diameter of the earth's orbit, and a geometrical calculation enables us to determine the proportion that the sides bear to this.

The method of determining parallax is theoretically so simple that it could not but be known to Copernicus and his followers. Indeed for three hundred years before Bessel's success there were painstaking attempts to apply it, attempts which invariably ended in the disappointing result that the two direction-lines from opposite ends of the earth's orbit always seemed to be parallel. We know this to mean that the star observed was too distant, or that the instruments used were not precise enough, to show appreciable parallax.

As we have noted, Bessel succeeded and the importance of the step thus taken is not affected by the fact that his estimate of the distance of 61 Cygni as 600,000 times that of the Sun is now reduced to 440,000.

A few months after Bessel announced his discovery, Henderson of Edinburgh published his estimate of the distance of α Centauri, which is, so far as we know, the star nearest the solar system. Henderson calculated its distance at 180,000 times that of the Sun, this has now been extended to 270,000 times.

Writing in 1885, Miss Clerke says: "The same work has since been steadily pursued, with the general result of showing that as regards their overwhelming majority, the stars are far too remote to

show even the slightest trace of optical shifting from the revolution of the earth in its orbit. In about a score of cases, however, small parallaxes have been determined, some certainly (that is, within moderate limits of error), others more or less precariously." *

Dr. Fison notes that for forty years after Bessel's discovery the record is chiefly one of accumulation of experiences; "and when in 1881 Dr. Gill and Dr. Elkin commenced a series of observations at the Cape of Good Hope, the parallaxes of not more than half a dozen stars had been detected with certainty. Since that date, however, parallax hunters have been better rewarded, though up to the present time (1898) it is doubtful whether success has been achieved in more than fifty instances." †

The distances of the stars whose remoteness is measurable are so enormous that they produce almost no impression on the ordinary mind.

"It follows," said Bessel, "that the distance of 61 Cygni from the sun is 657,700 times the half diameter of the earth's orbit. The light from the star takes ten years to traverse this enormous distance. It is so vast, that though it may be conceived, it cannot be visualised. All attempts to realise it, fail either because of the size of the unit of measurement or because of the number of repetitions of the unit. The distance which light traverses in a year is not more realisable than that traversed in ten years. Or if we choose a realisable unit, such as the distance of 200 miles which a locomotive (bicycle, we should say) travels in a day, it would require 68,000 millions of such daily journeys, or about 200

* A. M. Clerke. *History*, 1885, p. 48.

† *Recent Advances in Astronomy*, 1898, p. 7.

millions of yearly journeys to reach the star in question." *

It seems on the whole most convenient to use, as Bessel suggested, as a unit "*the light journey* of one year." The velocity of light is 186,300 miles a second, about six billion miles a year. "Light takes four years and four months to reach the earth from α Centauri, yet α Centauri lies some ten billions of miles nearer to us (so far as is yet known) than any other member of the sidereal system!" † In other words, we see α Centauri, not as it is now, but as it was more than four years ago. Similarly, light takes more than six years to reach us from 61 Cygni.

Given a determination of the parallax and distance of two stars in a system, and a knowledge of their period of revolution, it became possible to calculate their combined mass in terms of that of the sun; and the process of weighing the stars began.

Herschel's conclusion as to movement of the solar system as a whole, often doubted, was repeatedly confirmed; the general direction was more carefully stated; and even the rate has been guessed at. F. G. W. Struve (1793–1864) continued Herschel's study of double stars, and published in 1837 his monumental *Mensuræ Micrometricæ*, which "will for ages serve as a standard of reference by which to detect change or confirm discovery."

The distances of some of the nearer stars can be calculated by the determination of annual parallax, a method first successfully employed by Bessel (1838), *Henderson* (1839), *and Struve* (1840); *this is historically important as a confirmation of*

* Freely translated from Dannemann's *Grundriss einer Geschichte der Naturwissenschaften*, vol. 1, 1896, p. 325.

† A. M. Clerke. *History*, 1885, p. 49.

the Copernican system and as a suggestion of the sunlike nature of the stars.

Life of Stars.—If the view be accepted that the sun was once a diffused body of gas extending beyond the present limits of the solar system, and that it has slowly shrunk, giving rise to the present phase of things, and if the stars be regarded as sunlike, we should expect to find in the immensity of the heavens some confirmatory evidence. In other words, we should expect to see stars a-making and others a-dying. The former are now familiar to astronomers, and the existence of dead stars is generally admitted.

Nebulæ.—It is generally agreed that the faint clouds of light called nebulæ, which occur scattered in the sky, are in many cases at least early stages of star-making,—embryo stars in an undifferentiated state. Two of these nebulæ are visible to the unaided eye on clear dark nights, namely, in the constellations of Orion and of Andromeda.

In the seventeenth century, after Galilei had introduced the use of the telescope, many nebulæ were detected, but they were generally passed over quickly as "diffusions of self-luminous matter," or "shining fluid," or "fire-mist," and so forth. Towards the end of the eighteenth century (1780) William Herschel began his study of nebulæ, and not only increased the list from 150 to 2,500 in about a score of years, but showed that many of them had a detailed structure. At first he regarded nebulæ as clusters of stars, and stated the evolutionary idea that stars and clusters of stars arose from nebular condensations. Subsequently, however, he reverted to the older view in regard to many nebulæ, including that of Orion. In the first half of the nineteenth century it was Herschel's earlier view that prevailed;

improved telescopes, such as that constructed by Lord Rosse at Parsonstown in Ireland, resolved one nebula after another into collections of stars. Indeed imagination far outstripped the evidence, and it was widely supposed that nebulæ were systems of suns, multiples, as it were, of the architectural unit which our solar system was believed to display.

So far telescopic analysis had alone been possible, but the next great step was taken in 1864, when Sir William Huggins applied the spectroscope to the study of a small but bright nebulæ in the constellation of the Dragon. The spectrum (yielding no continuous band) was like that of a glowing gas, and therefore it was concluded that this nebulæ was not a galaxy of stars, but a vast area of incandescent gas. In the next few years many others, including the Great Nebulæ of Orion, were shown to be gaseous while others (yielding "continuous" spectra) seemed to be either star clusters or gases in process of condensation.

It is important to notice that the growth of thermodynamics has led to a rejection of the old view that nebulous stuff was originally or is still "instinct with fire." The essay of Helmholtz in 1854 made it plain that this supposition is unnecessary, "since in the mutual gravitation of widely separated matter we have a store of potential energy sufficient to generate the high temperature of the sun and stars. We can scarcely go wrong in attributing the light of the nebulæ to the conversion of the gravitational energy of shrinkage into molecular motion." *

"*It is difficult not to see in the gaseous nebulæ the stuff of which future stars will be made. Granting that their substance is subject to the law of gravi-*

* Huggins. *Rep. Brit. Ass.* for 1891, p. 22.

tation, it appears certain that in coming ages their glowing matter must, under its influence, be drawn towards centres of condensation; the smaller and more symmetrical of the nebulæ possibly developing into single stars, but such majestic collections of cloudy structures as are revealed in Orion being more probably the origin of hosts of separate suns."

Dead Stars.—While some nebulæ are plausibly interpreted as stars a-making, there are also phenomena which indicate stars dying or dead, or in other words, dark. It is obvious that the existence of a dark star cannot be demonstrated to the eye; but it may be inferred (*a*) from the occurrence of the total or partial eclipse of a bright star, or (*b*) from disturbances in the movement of a bright star such as the gravitational influence of a dark neighbour would explain. In both these ways the existence of dark stars has been indirectly proved.

The regularity in the variations of the light of Algol—the best-known of the variable stars—was hypothetically interpreted by Goodriche (1782) as due to the revolutions of a dark companion star which caused partial eclipse; and the researches of E. C. Pickering of Harvard (1888) and of Vogel of Potsdam (1888–1891) have justified the hypothesis.

"*The possibility of an unseen system of stars permeating the seen is beyond doubt.*" *

Condensing Dr. Fison's account of the subject, we may sum up the possible history of a nebula. A diffused area of gases, perhaps comparatively cool, perhaps holding part of its contents in the form of solid or liquid particles; gravitational attraction brings about a spherical form; heat is lost by radiation and the parts of the area draw together; tem-

* Fison. *Recent Advances*, p. 35.

perature rises and the nebula becomes more thoroughly gaseous, if it was not so at the start; as the outer parts cool they condense into the clouds of a photosphere and the nebula becomes a sun; for a time, as shrinkage increases, the temperature rises; but the limits to this must be reached sooner or later and the sun, passing the zenith of its splendour, gradually sinks into dark coldness.

"*Fixed Stars.*"—One of the many instances of the characteristic nineteenth century transition from static to kinetic conceptions, may be found in the hesitancy with which astronomers now speak of "fixed stars." In many cases it has been shown that their positions relative to one another change in the course of years, and the displacement, though apparently very minute, indicates an enormous velocity of movement. "Sirius drifts over the face of the sky with such speed that in 1,400 years its position will be removed from its present one by a distance that would just be covered by the diameter of the full moon. . . . To do this it must travel athwart the direction of vision with a speed of over ten miles per second, more than one-half of that of the Earth in its orbit; and this takes no account of any velocity the star may possess in the direction of the line of vision, a displacement in which direction would obviously not affect its position upon the face of the heavens." *

Similarly, to take the most rapid known displacement, a star in the Great Bear named Groombridge, 1830, would move in 257 years over the moon's diameter, and this at a distance of 2,300,000 times that of the sun implies a rate of 227 miles per second. Nor should we forget here that the sun itself is travelling in a line directed towards the star Vega,

* Fison, p. 46.

at a rate which some estimate at 12–18 miles per second. There has been no justification of the hope of a century ago that some star (Sirius was suggested by Kant) or some point (in the Pleiades, according to Mädler) would turn out to be the hub of the universe, the centre to which all the heavenly bodies are related; the system or goal of the grandest of all movements is unknown.

EXTENSION AND INTENSIFYING OF OBSERVATION.

Extension of Observational Astronomy.—In astronomy, as in other sciences, a large part of the available intellectual energy has gone and must go to extend the area of observation, or to revise with intensified carefulness what has been already observed. It is difficult to give any account of this ungeneralised work, whose value is in the future rather than in the present. Numbering the stars is like cataloguing Radiolarians or Diatoms, a means not an end; and a telescopic photograph of a corner of the Milky Way is like a similar picture of a microscopic section—interesting and marvellous, of course, for everything is—but not attaining full interest until it can be used as an item in some generalisation.

The Milky Way.—To take an instance: The Milky Way—the high road to Olympus—has been the subject of imaginings since men first saw the stars. Its poetical interpretations are many, but as to its scientific interpretations there has been little progress since Galilei's telescope confirmed the conjecture of Pythagoras that the haze of the dimly luminous arch was "the combined shimmer of hosts of stars, each one too faint by itself to be distinguished by the unaided eye."

Both the Herschels, Struve, Proctor, and others sought to explain the appearance of this majestic way of light as due to perspective effect or optical projection, but there seems to have been a complete acceptance of "the more simple and direct view, that the Milky Way is a definite and complicated structure, and that its bifurcation, its vacuities, its gaps, and its other irregularities, are definite physical facts." *

The great "Bonn Durchmusterung" compiled (1859–1862) under the supervision of Argelander, the more recent Harvard catalogue by Pickering, and Gould's list of the stars visible from the southern hemisphere, illustrate supreme patience and carefulness, but as yet we remain unaware of any securely established or intelligible generalisations as to the stellar distribution. The Bonn list includes 324,198 stars down to a certain (9.5) magnitude (estimated in terms of brightness), but mere number does not impress the imagination, especially since the sight of the starlit sky suggests legions upon legions of luminaries visible to the unaided eye,—a suggestion very far from the truth. The more impressive aspect is that which remains vague, of which, indeed, we have as yet only suggestions, that there is probably a system of the stars, —hidden from our gaze not only by distance, but by its inherent complexity.

A quotation from one of the modern masters may serve to suggest the present tentative position:—"The heavens are richly but very irregularly inwrought with stars. The brighter stars cluster into well-known groups upon a background formed of an enlacement of streams and convoluted windings and

* Fison. *Recent Advances*, 1898, p. 85.

intertwined spirals of fainter stars, which become richer and more intricate in the irregularly rifted zone of the Milky Way.

"We, who form part of the emblazonry, can only see the design distorted and confused; here crowded, there scattered, at another place superposed. The groupings due to our position are mixed up with those which are real.

"Can we suppose that each luminous point has no other relation to those near it than the accidental neighbourship of grains of sand upon the shore, or of particles of wind-blown dust of the desert? Surely every star from Sirius and Vega down to each grain of the light-dust of the Milky Way has its present place in the heavenly pattern from the slow evolving of its past. We see a system of systems, for the broad features of clusters and streams and spiral windings which mark the general design are reproduced in every part. The whole is in motion, each point shifting its position by miles every second, though from the august magnitude of their distances from us and from each other, it is only by the accumulated movements of years or of generations that some small changes of relative position reveal themselves.

"The deciphering of this wonderfully intricate constitution of the heavens will be undoubtedly one of the chief astronomical works of the coming century." *

One interesting result as to method should be noted, namely, the development of stellar photography. When even the trained eye, with the telescope to help, cannot detect, the photographic plate may reveal. The invention and improvement of the gela-

* Sir William Huggins. President's Address, *Rep. Brit. Ass.* for 1891, pp. 35–36.

tine dry plate, which on sufficiently long exposure will register an image of a body whose luminosity falls far below the limit of visibility to our eyes, has meant a remarkable extension of our sense of sight. It has meant seeing the invisible!

Of some importance, too, has been the development of more exact methods of measuring star brightness (photometry), and the resulting classification (first suggested by Pogson in 1856) into definite degrees of "magnitude." Thus a star of the sixth magnitude is one hundred times fainter than one of the first magnitude.

Intensifying of Observation.—Inspection of the recent moon-maps and photographs, as seen, for instance, at the Paris Exposition (1900), will illustrate what is meant by an *intensifying* of observation.

The Moon.—The large size of our satellite (2,160 miles in diameter), and its relative nearness to us (238,833 miles from the earth's centre), facilitated the careful study of its superficial characters, at least of that side which is alone presented to our view. The systematic and interpretative study of the moon's face practically began with the century, for it dates from Schröter's *Solenotopographische Fragmente* (1791–1802). Lohrmann and Schmidt, Beer and Mädler, Nasmyth and Carpenter, Neison and Secchi, and many more have added detail to detail, so that it is safe to say there is no country mapped so nearly up to the present limits of possible precision. The heights of some of its mountain ranges have been computed from their shadows and the depths of some of its extinct craters have been sounded. We have certainly advanced far from the older view, which even Herschel did not entirely get

rid of, that the moon might be habitable like the earth, and yet there seems no unanimous answer to the question:—Has the moon no atmosphere, or one of extreme tenuity? We have got far from the belief of Schröter, who imagined he had discovered a lunar city; what were called seas are now said to be covered with dry rock; what are called rills are now said to be great clefts or gorges certainly waterless, but we remain in doubt as to the meaning of the broad white rays which diverge for hundreds of miles from some of the principal "ring-plains," and there are many who attribute to glaciation what others confidently interpret as due to volcanic action. Perhaps the most interesting observations are the few which point —though with insufficient security—to some slight changes on the moon's apparently changeless face.

Similarly, there are maps of Mars now in circulation, which surpass in detail those available in regard to Africa a century ago. And though the precision of these Martian maps may be fallacious the same is true of many of the early maps of Africa, and we cannot gainsay the impression of a greatly increased intensity of observation. To what is this due? To more powerful telescopes, to the use of the spectroscope and polariscope, to the development of photography, and to an exact knowledge of the times (in "opposition" to the sun, i. e., nearest the earth) when Mars can be studied to best advantage.

The study of Mars illustrates the growing intensity of observational study, while the imaginary superstructure reared by some on the supposed existence of an intricate system of canals illustrates the danger of outstripping the evidence.

PHYSICAL AND CHEMICAL PROBLEMS.

Beginnings of Physical Astronomy.—In 1610,

Fabricius and Galilei discovered sun-spots, which are still of fascinating interest to astronomers. In early days, some regarded them as due to the transits of small planets across the sun's disc, others thought of them as clouds, others as masses of cindery slag in process of being sloughed off, and so on. In 1774, Prof. Alexander Wilson of Glasgow was able to give geometrical definiteness to the suggestion, which had been repeatedly made, that the spots were due to great excavations in the sun's substance. He also expounded the idea, which William Herschel elaborated, that the sun was like an earth within, but surrounded by an aurora of resplendent clouds. Some estimate of the state of knowledge in regard to the physical constitution of the sun may be got from Sir William Herschel's eloquent descriptions about the beginning of the nineteenth century. It was to him a sort of glorified earth, with hills and valleys, luxuriant vegetation, and a population, protected by a cloud-canopy from a radiant outer shell some thousands of miles in thickness. This "was nothing less than the definite introduction into astronomy of the paradoxical conception of the central fire and hearth of our system as a cold, dark, terrestrial mass, wrapt in a mantle of innocuous radiance—an earth, so to speak, within—a sun without." * Herschel's authority gave vitality to this conception, whose main utility was that it helped to definitise error—often the first step to its demolition. But it would be historically unjust to ignore the fact that although Herschel's main idea was quite erroneous, it was the peg to which a number of accurate observations were temporarily attached.

* A. M. Clerke. *History*, 1885, p. 71.

William Herschel's picture of the sun seems to have been generally accepted for about seven decades. His son, Sir John Herschel, while working at the Cape, was probably beginning to doubt its validity when he maintained that the sun's rotation was intimately concerned with the formation of sun-spots; and the attention which he, Baily, Airy, Arago, Struve, and others paid to the corona, chromosphere, and other luminous appendages of the sun observed during the eclipses of 1842 and 1857, led to further suspicions.

The careful patience of an amateur—Heinrich Schwabe (d. 1875)—made the next step possible, for by the observations of a quarter of a century he showed, about 1850, that there was a periodicity in the appearance of sun-spots. But this, in itself interesting, acquired additional importance when the magnetic observations which the enthusiasm of Humboldt, Gauss, and others had secured in five continents led Dr. John Lamont and Sir Edward Sabine (1852) independently to the conclusion (based on different sets of data), that there was a remarkable harmony between periods of disturbance in terrestrial magnetism and the periods of the sun-spots. The congruence was confirmed in the same year (1852) by Rudolph Wolf and by A. A. Gautier, and although Sir William Herschel's association of the price of bread, periods of sunny weather, and frequency of sun-spots was not borne out, the influence of the sun on the earth's magnetism was henceforth recognized as a fact.

It is now generally believed that the sun is surrounded by a halo of incandescent clouds—the photosphere—outside of which there is a solar atmosphere composed of vapours of hydrogen, calcium, iron, and

other metals, besides a few non-metallic elements. The clouds of the photosphere may be due to fog-precipitates from the cooling atmosphere, while depressions or gaps in the photosphere probably give rise to the phenomena of sun-spots. Herschel's idea of a solid core—cool and even habitable—gave place to the idea of an ocean of molten matter, but this, with fuller knowledge of the conditions of the various states of matter, has given place to the generally accepted view that the sun is in the main or wholly gaseous.

The Sun's Heat.—About 1836, Sir John Herschel at the Cape and Pouillet in France took a step which meant much to the progress of physical astronomy. It is hardly necessary to say that the step was one of measurement. They tried to measure how much of the sun's radiant energy is intercepted by the earth—a mere speck in the heavens (one part in two thousand millions!) Although their estimates were afterwards shown, by the work of Young, of Langley (1880–81), of Janssen (1897), and others to be far under the mark, they were sufficient to indicate the magnitude of the flood of energy which pours forth from the hearth of our system.

Herschel calculated that the heat received by the earth in a year (including that caught in the atmosphere) would suffice to melt a covering of ice 120 feet thick over the whole surface of our planet; Young's estimate leads to the result that "each square metre of the Sun's surface pours out enough heat to maintain about half a dozen mighty Atlantic steamers at their utmost speed night and day, from year's end to year's end;" * Langley remarks that "though there

* Sir Robert Ball, *The Story of the Sun*, 1893, p. 263.

is coal enough in the State of Pennsylvania to supply the wants of the United States for many centuries to come, yet the heat which could be generated by the combustion of all the coal in Pennsylvania would not be sufficient to supply the sun's radiation for the thousandth part of a single second." *

From experiments on the intensity of the radiation emitted by an incandescent body, Le Chatelier has argued (1892) that the temperature of the sun cannot be less than 7,600° C., and probably much more. These and similar figures convey little meaning in themselves, but they are significant in relation to the problem of how the supply of energy is sustained.

Maintenance of Solar Energy.—Especially after the formulation of the doctrine of the conservation of energy (about 1843), the problem of the maintenance of the sun's heat urgently claimed attention. It soon became evident that it is impossible to think of the sun as like an enormous fire giving out heat by combustion. "Massive as the sun is, if its materials had consisted even of the very best materials for giving out heat by what we understand on the terrestrial surface as combustion, that enormous mass of some 400,000 miles in radius could have supplied us with only about 5000 years of the present radiation." † From what we know of the sun's age and the amount of its radiation, it is certain that its heat cannot be mainly due to chemical processes at present known to us.

Setting aside the chemical solution of what Sir John Herschel called "the great secret," we find two

* Sir Robert Ball, *The Story of the Sun*, 1893, p. 265.
† P. G. Tait, *Recent Advances*, 1876, p. 151.

other suggestions. About 1848, Mayer, who shared in stating the idea of the conservation of energy, brought forward a "meteoric hypothesis" according to which it was supposed that the meteorites swarming around the sun engendered heat by impact with it,—thus furnishing a supply of heat many thousand times greater than if they underwent complete combustion. This view, also suggested by Waterston, was developed in 1853 by Sir William Thomson (Lord Kelvin) and was supported by Tyndall and Tait. The latter says: "We find, by calculations in which there is no possibility of large error, that this hypothesis is thoroughly competent to explain 100,000,000 of years' solar radiation at the present rate, perhaps more; and it is capable of showing us how it is that the sun, for thousands of years together, can part with energy at the enormous rate at which it does still part with it, and yet not apparently cool by perhaps any measurable quantity." *

On the other hand, while the infall of meteorites and the heat they produce by impact may be regarded as certain, it is urged by competent authorities that the "intra-planetary" supply is too scanty to be more than a makeshift, while Lord Kelvin himself excluded an "extra-planetary" supply on the ground that if it were true the year would be shorter now by six weeks than at the opening of the Christian era.†

In 1854, Helmholtz gave the answer which is now generally accepted. If we start with the reasonable assumption of a once larger and less condensed sun, we can understand that as the sun shrank there was thereby accumulated a great thermal store

* *Recent Advances*, 1876, pp. 153–54.

† See Miss Clerke's *History*, p. 352.

—the direct result of the condensation. Most of this has already been lost; but as the cooling proceeds, further condensation of the interior (gases) ensues, and this implies further evolution of heat. Thus as the sun parts with heat it compensates for its loss by evolving more. In brief, gravitational energy is exchanged for radiant energy. How long it can continue to do so before ceasing to glow, before fading away into a dark star, is really indeterminable in the present state of our knowledge of the sun's physical constitution, but some rough calculations have been made. Helmholtz estimated the rate of the sun's contraction at about 220 feet a year, and granted a lease of life for many millions of years to come.

Whether the sun is at present becoming actually cooler we do not certainly know, but it is interesting to take note of Lane's theorem (1870), which, on the assumption that the sun is gaseous and behaves as a perfect gas (one whose relations of volume to pressure are indicated by Boyle's Law), seeks to show that the temperature must be increasing, not decreasing. As we cannot assert that the behaviour of gases in the sun's interior is such as Boyle's Law indicates, we cannot at present decide whether the sun has yet attained its maximum splendour or whether it has now begun to wane.

Collisions and Impacts.—From what has been said it is evident that the picture of the sun's origin which astronomers incline to give, is that of a vast primitive nebula, with a great store of energy in the mutual gravitation of its parts. We have also noted the importance of the suggestion due to Helmholtz—that cooling induced shrinkage, and that this in turn evolved more heat. But another possible

factor in the production of the sun's heat has been suggested by several astronomers.

Sir Robert Ball illustrates this by the story of the new star in Auriga, whose appearance was observed in February, 1892. Where a few days before the photographic plates had shown nothing, a bright star suddenly became apparent. "Everything we have learned about the matter suggests that the new star in Auriga during the time of its greatest brilliance dispersed a lustre not inferior to that of our own sun. . . . It became clear that the brightness of the new star in Auriga was the result of a collision which had taken place between two previously obscure bodies. Perhaps it would hardly be right to describe what happened as an actual collision. It is, however, perfectly clear from the evidence that two objects, whose relative velocities were some hundreds of miles to a second, came into such close proximity that by their mutual action a large part of their energy of movement was transformed into heat, and a terrific outburst of incandescent gases and vapours proclaimed far and wide throughout the universe the fact that such an encounter had taken place." *

From the analogy of *Nova Aurigæ*—which is no isolated instance—it has been conjectured, by Lord Kelvin among others, that our sun may have arisen from the collision of two bodies which attracted each other until they became a single sun with an enormous store of heat derived from the crash of their impact.

This speculation is of interest when we look forward to the time in the life of a sun or star, when further compression no longer compensates for the

* Sir Robert Ball, *loc. cit.*, p. 277.

loss of heat by radiation. There seems then no possibility of the star recovering itself, unless through a collision with another. For it is possible that the heat produced by the impact might restore them to the primitive nebulous state. If the two colliding bodies were solid the result might be a shattering into fragments which would be projected with high velocities into space; but if the stars had not cooled enough to be solid, fragmentation would be less likely, and the collision might lead to rejuvenescence.

The establishment of stellar physics practically dates from the application of the spectroscope to the investigation of the composition of the sun, the planets, and the stars. The facts illustrate what has been repeatedly true in the history of science, that the application of a new instrument or method, may lead to development at a rate and in a direction which no one would have ventured to predict.

SPECTRUM ANALYSIS.

The spectroscope is a combination of prisms (or equivalent structures such as a "diffraction-grating") by means of which the various rays composing a particular kind of light can be separated out and arranged in a line, the differences of wave-length showing themselves as differences of colour. Thus the presence or absence of certain kinds of light can be seen at a glance. The use of the instrument in astronomy is based on the facts (1) that the quality of light is not affected by distance; (2) that each element when in a glowing state emits characteristic rays of light or has a definite discontinuous spectrum; and (3) on what is known as Kirchhoff's law of selective absorption. Thus the spectroscope

furnishes a means of showing that certain kinds of glowing matter—known to our terrestrial experience—also occur in sun and stars. But the recognition of the importance of this new organon came about very gradually.

Gradual Discovery.—In 1672 Sir Isaac Newton made the simple but beautiful experiment (which Kepler had also tried less effectively) of using a prism to split up a ray of sunlight which entered a darkened room through a round hole bored in the shutter. He thereby produced a spectrum or image of the differently coloured constituents of light, due, as he showed, to the fact that these constituents (rays of different wave-length, as we now say) have different refrangibilities. This was the beginning of the analysis of sunlight, which was destined to have such a remarkable future.

The historians tell us that a young Scotchman Thomas Melvil (d. 1753) began the study of the spectra of salts, and the spectroscope was certainly a chemist's instrument before its astronomical value was recognised. It may be recalled that several elements—cæsium, rubidium, thallium, indium, gallium, and scandium were discovered by means of the spectroscope. In 1802, Wollaston replaced "the round hole in the shutter" by a fine slit parallel to the edge of the prisms, showed that there were gaps in the solar spectrum, and made the further important step of contrasting the spectrum of sunlight with that of a candle flame.

Mechanical improvements were soon introduced by Fraunhofer (1814) and Simms (1839). Fraunhofer, independently of Wollaston, also mapped out a large number of the dark lines in the spectrum of sunlight, and called particular attention to the fact

that two adjacent yellow lines in the spectrum of a candle flame (now known to be due to sodium) coincided with a pair of dark lines in the solar spectrum. Similarly Brewster showed that the potassium lines coincide with other Fraunhofer lines.

In 1822 Sir John Herschel noted the bright lines of flames in which certain metallic salts are burnt, and in 1825, along with Talbot, he suggested the importance of using the spectroscope to detect the presence of minute quantities of certain substances in minerals. In 1826 Talbot almost reached the fundamental conclusion that the presence of a certain line in the spectrum tells unerringly that a certain substance is glowing in the fire of the luminous body. Brewster followed on the same track, and William Swan noted the delicacy of the spectroscopic test in detecting the presence of various substances, such as common salt.

As we have already hinted, gaps or dark lines in the solar spectrum mean that rays of a certain refrangibility (which depends upon wave-length) are absent. It is plain that they may be absent from the start or simply because they are absorbed in passing through the earth's atmosphere. Thus it was an important step when, in 1832, Sir David Brewster noted that some of the dark lines which Fraunhofer had mapped out on the solar spectrum, were intensified when the sun was near the horizon, that is to say when its rays have a longer path through the earth's atmosphere and are therefore more liable to absorption. Gaps thus due to absorption by the earth's atmosphere are called "telluric lines."

The coincidence noted by Fraunhofer between two yellow lines on the sodium spectrum and a pair of dark (D) lines in the solar spectrum, was carefully

tested by Professor Miller; and Sir Gabriel Stokes suggested in 1850, as Angström did in 1853, that the double D line must be due to the absorptive action of sodium vapour in the sun's atmosphere. Interesting also in this connection was Swan's explanation that the appearance of the two yellow sodium lines in all sorts of flames was due to the almost universal distribution of common salt (sodium chloride) in the earth's atmosphere.

In 1849 Foucault had shown, without seeing the importance of the fact, that the D lines were darkened when the sunlight was passed through an electric arc which gave bright sodium lines in its spectrum. It was reserved for Kirchhoff ten years later to show clearly what this meant.

Thus spectrum analysis "has grown out of some apparently insignificant and disconnected observations made by Marcgraf, Herschel, and others upon the light emitted by flames coloured by certain salts. The spectra of such flames were investigated by various physicists, among whom Talbot, Miller, and Swan deserve first mention; but it was only after Kirchhoff (in 1860) had made and proved the definite statement that every glowing vapour emits rays of the same degree of refrangibility that it absorbs, —that spectrum analysis became developed by Bunsen and himself into one of the great branches of science." * Again we find an illustration of the historical fact that apparently trivial beginnings often lead to great issues, and should never be judged hastily.

Bunsen and Kirchhoff.—These two investigators were the first to show conclusively that definite

* E. von Meyer. *History of Chemistry.* Trans. 1891, p. 445.

bright lines in the spectra of various flames are due to the presence of definite glowing vapours in these flames. In other words the presence of certain lines in the spectrum is a sure index of the presence of certain elements in the luminous body.

In a famous experiment, Kirchhoff and Bunsen interposed the flame of a spirit lamp, on whose wick some salt had been sprinkled, in the line of the rays from a lime-light, and found that on what would have been a continuous spectrum there were two *dark* sodium lines—the phenomenon of "reversal." Yet when the salted flame of a Bunsen burner was substituted for that of the spirit lamp, the "reversal" phenomenon did not occur, but a bright yellow pair of lines was superposed on the lime-light spectrum. Thence they inferred that to effect "reversal" the temperature of the vapour through which the light passes must be less than that of the radiating source —a conclusion afterwards developed by Balfour Stewart, and of great importance in the study of the solar spectrum. For it led investigators to recognise that the appearance of dark lines in the spectrum of the sun implies that the gases in the sun's atmosphere must be at a lower temperature than those in the photosphere behind.

Kirchhoff's Law.—The experiment of the reversal of the lines was the concrete proof of what Kirchhoff had reached mathematically—the law of selective absorption—which was also approached by Angström and Balfour Stewart.

"The law states that the ratio between the emissive power and the absorptive power is the same for all substances at the same temperature for rays of the same wave-length. From this it follows that all opaque substances begin to glow at the same temperature—that is,

that they give out light of the same wave-length—and that incandescent substances only absorb such rays as they themselves emit. Since, however, incandescent gases possess maxima and minima of light intensity, while solid and liquid substances emit light of every kind when sufficiently heated, the former must also possess a selective absorptive power, and this is not the case in general with the latter. The Fraunhofer lines are thus explained as consequent upon absorptions by incandescent vapours." *

Applications.—From the coincidence of the two yellow sodium lines in the spectrum of a candle flame with two of Fraunhofer's dark lines in the solar spectrum, Kirchhoff concluded that sodium was present in the sun's atmosphere; and the same kind of argument was used over and over again. The method is to find in the spectra of terrestrial elements bright lines which exactly coincide with the dark lines in the sun's spectrum. Thus Kirchhoff showed that besides sodium, the sun's atmosphere contained iron, calcium, magnesium, nickel, barium, copper, zinc, and chromium, while others such as gold and silver were similarly shown to be absent. In 1852 Angström added hydrogen and others to the list; in 1872–1876 Lockyer increased the number from 14 to 34; in 1887 Trowbridge and Hutchins demonstrated the presence of carbon; in 1891 Rowland detected silicon. The absence of some elements, notably of oxygen, is as remarkable as the presence of others, but there is, as Lockyer and others have shown, some reason to suspect that elements may be present when they are apparently absent; that is to say they may exist under physical conditions which

* Ladenburg. *History of Chemistry.* Trans. by Dobbin, 1900, pp. 317 to 318.

disguise or modify their spectrum, or they may perhaps be "dissociated" into more elementary forms of matter.

In short, the date 1859 *or* 1860 *marks the widening of astronomy from being a science descriptive of movements to be also a science descriptive of the chemical constitution and changes of the heavenly bodies.*

Extension to the Stars.—There is no greater triumph of scientific analysis than that by which a minute beam of sunlight has been made to disclose the chemical constitution of the sun's atmosphere, and this, as we have seen, was the first general result of the application of the spectroscope to astronomy. But what can be done with sunlight can also be done in some measure with starlight, and the application of the spectroscope to the stars has been one of the characteristic features of the astronomical work of the second half of the nineteenth century.

As early as 1814, Fraunhofer observed that the dark lines of stellar spectra, though sometimes agreeing with those in the sun's spectrum, were oftener different, both in arrangement and intensity; but it was with Kirchhoff's researches that the spectroscopic study of the stars began in earnest. About 1863 Sir William Huggins and Dr. Miller began the systematic study of stellar spectra, and the former extended his observations to nebulæ, showing that some of these (with a spectrum of bright lines) are not star-clusters but areas of incandescent gas. As early as 1864, Huggins was able to identify some of the dark lines in the spectra of stars with those of known elements, such as hydrogen, iron, sodium, and calcium,—a kind of work which has since been vigorously prosecuted.

But while the use of the spectroscope revealed the presence of certain chemical elements in the stars, and distinguished gaseous from star-cluster nebulæ, it led to an even more important achievement—the detection and measurement of the motion of certain stars in the line of sight. We cannot briefly explain the suggestion of Christian Doppler (1848) that "the colour of an object should be affected by the motion of the source, becoming more violet as the object approached, and inclining toward red as it receded from, the observer," * or the method of Fizeau (1848) by which the displacement of the dark lines in the spectrum was used as an index of approach or recession. These led to the work of Sir William Huggins who announced in 1868 that he had found spectroscopic evidence (a minute displacement of a dark hydrogen line) of the recession of Sirius and estimated the rate of this recession (from the sun) at 29½ miles per second. He extended the discovery to thirty other stars and confirmed the method by the spectroscopic study of Venus at different times—when the planet was known to be moving towards or away from the earth.

It is interesting to notice that displacement of lines has also been detected in the observation of sun-spots, and has led to the conclusion that these are due to downrushes of gases.

From 1870 onwards, the splendid work of Huggins was continued by Hermann Vogel, at Potsdam, who in 1887 availed himself of the valuable aid afforded by the dry gelatine plate and the microscopic examination of its photographic record of the spectrum. The motions of approach and recession of many stars were thus calculated with great accuracy, and

* Fison. *Recent Advances*, 1898, p. 200.

this is only one of many results with which spectrum analysis has enriched astronomy. Thus we might refer to the remarkable argument from spectroscopy which led Pickering of Harvard in 1889 to infer that a certain star in *Ursa* was really double, or Vogler to confirm the suggestion that the variability of Algol was due to its being periodically eclipsed by a dark or nearly dark companion star. In short, besides chemical information, the spectroscope affords a means of determining celestial motions in the line of sight, and has detected binary which the telescope could never have revealed.

Sir William Huggins writes: "In no science, perhaps, does the sober statement of the results which have been achieved appeal so strongly to the imagination, and make so evident the almost boundless powers of the mind of man. By means of its light alone to analyse the chemical nature of a far distant body; to be able to reason about its present state in relation to the past and future; to measure within an English mile or less per second the otherwise invisible motion which it may have towards us or from us; to do more, to make even that which is darkness to our eyes light, and from vibrations which our organs of sight are powerless to perceive, to evolve a revelation in which we see mirrored some of the stages through which the stars may pass in the slow evolutional progress—surely the record of such achievements, however poor the form of words in which they may be described, is worthy to be regarded as the scientific epic of the century." *

The extension of spectrum analysis to the stars has yielded information as to the chemical elements which occur in them, has distinguished gaseous neb-

* President's address. *Rep. Brit. Ass.* for 1891, p. 4.

ulæ from star-clusters, has afforded a method of measuring the motions of stars in the line of sight, and has led to many other results which afford fine historical illustration of the value of co-operation between sister-sciences.

THE EVOLUTION-IDEA IN ASTRONOMY.

The evolution-idea has asserted itself in astronomy especially in connection with what is called the nebular hypothesis,—an attempt to give an account of the origin of a solar system. It is said to have arisen as a transcendental conception in the mind of Swedenborg; it was suggested on general grounds by Kant; it was formulated in mechanical terms by Laplace; and it has been the subject of much discussion—on the whole unfavourable to its details, though confirmatory of the general idea.

It was in 1755 that Immanuel Kant (1724–1804) published his *General Natural History and Theory of the Heavens,* more than a quarter of a century before his *Critique of Pure Reason.* Based, as its title indicates, on Newton's *Principia,* the essay pictures a possible mode of origin for the sun and the planets from a homogeneous distribution of vaporous particles in the space now occupied by the solar system.

A more important step was taken in 1796 when Laplace presented his "Nebular Hypothesis." Starting from a vast fluid nebula in slow rotation, he supposed that as this cooled it contracted, that as it contracted its rate of rotation increased, that eventually the "centrifugal force" of the great nebular sphere exceeded the centripetal gravitational attraction, and a nebulous ring was separated off from the

equatorial regions. This ring afterwards broke up, but its parts condensed to form the furthest planet. With further shrinkages and accelerations of the parent nebular mass, the various planets were thrown off in succession, themselves to repeat the process in forming rings like Saturn's, or satellites like those of Jupiter.

One of the chief reasons which led Laplace to think out a possible unity of origin for the solar system, was that the planets and their satellites revolve and rotate in the same direction as that in which the sun rotates,—a coincidence of many (40 or more) motions which almost suggests a common origin. We now know that the satellites of Uranus and Neptune move in the opposite direction, and that there are other exceptions, e.g., that the inner Martian moon revolves in a shorter time than Mars, to the uniformity which Laplace proposed; on the other hand we know that there are many more instances of uniformity of motion than he was aware of.

There are many other sets of facts which favour the general *idea* of the nebular hypothesis. Thus we have a rapidly increasing mass of information in regard to the nebulæ which Herschel was the first to begin to study in earnest, some of which look like the primeval nebula which Laplace postulated, while others present appearances suggestive of systems in process of being made. The great Nebula in Andromeda, as photographed by Roberts, "suggests," as Huggins says, "a stage in a succession of evolutional events not inconsistent with that which the nebular hypothesis requires."

That the same substances occur (as the spectroscope proves) in sun and planets is another fact which

would fit in well with the evolutionary theory, being suggestive of community of origin.

Corroboration may also be found in Helmholtz's shrinkage theory (previously noted) of the origin and maintenance of solar energy, for it leads us back to a larger and less condensed sun, and thence to one larger still, until finally we approach something like Laplace's primitive nebula. "We can reason back to the time when the sun was sufficiently expanded to fill the whole space occupied by the solar system and was reduced to a great glowing nebula. Though man's life, the life of the race perhaps, is too short to give us direct evidence of any distinct stages of so august a process, still the probability is great that the nebular hypothesis, especially in the more precise form given to it by Roche, does represent broadly, notwithstanding some difficulties, the succession of events through which the sun and planets have passed." *

"So little is, however, known of the behaviour of a body like Laplace's nebula when condensing and rotating that it is hardly worth while to consider the details of the scheme, and that Laplace himself did not take his hypothesis nearly so seriously as many of its expounders, may be inferred from the fact that he only published it in a popular book, and from his remarkable description of it as 'these conjectures on the formation of the stars and of the solar system, conjectures which I present with all the distrust which everything which is not a result of observation or of calculation ought to inspire.'" †

Meteoritic Hypothesis.—We have already alluded to the speculation, which is now particularly asso-

* Sir W. Huggins. *Rep. Brit. Ass.* for 1891, p. 20.

† Arthur Berry. *Short History of Astronomy*, 1898, p. 322.

ciated with the names of Faye and Sir J. Norman Lockyer, that crowds of discrete meteoric bodies drawn together into aggregates by gravitational attraction, and evolving heat by collisions, may have given rise to nebulæ, with further condensation to luminous stars, and eventually to dark planets, whose vitality is at an end unless a collision make it possible for the evolutionary process to recommence. But this remains in the speculative phase.

The possibility, however, must be borne in mind that some of the existing nebulæ may have originated in the collisions of dark suns, and are thus the children, as it were, of a later generation. "During the short historic period, indeed, there is no record of such an event; still it would seem to be only through the collision of dark suns, of which the number must be increasing, that a temporary rejuvenescence of the heavens is possible, and by such ebbings and flowings of stellar life that the inevitable end to which evolution in its apparently uncompensated progress is carrying us can, even for a little, be delayed. . . . We cannot refuse to admit as possible such an origin for nebulæ." *

Tidal Friction.—An interesting recent contribution to the theory of the evolution of planetary systems, and of satellites in particular, has been made by Mr. G. H. Darwin, in his papers on the influence of tidal friction, but the subject is too intricate for discussion within our limits.

SUMMARY.—*A cautious summary forms the last paragraph of Berry's* Short History of Astronomy, *and this we venture to quote:—" Speaking generally, we may say that the outcome of the nineteenth-century study of the problem of the early history of the*

* Sir W. Huggins. *Rep. Brit. Ass.* for 1891, p. 24.

solar system has been to discredit the details of Laplace's hypothesis in a variety of ways, but to establish on a firmer basis the general view that the solar system has been formed by some process of condensation out of an earlier very diffused mass bearing a general resemblance to one of the nebulæ which the telescope shows us, and that stars other than the sun are not unlikely to have been formed in a somewhat similar way; and, further, the theory of tidal friction supplements this general but vague theory, by giving a rational account of a process which seems to have been the predominant factor in the development of the system formed by our own earth and moon, and to have had at any rate an important influence in a number of other cases."

CHAPTER VII.

Growth of Geology.*

CATACLYSMAL, UNIFORMITARIAN, EVOLUTIONARY.

These are cumbrous words for the heading of a paragraph, and yet they are serviceable to sum up the three chief phases of geology during the nineteenth century. For if it be borne in mind that phases of science do not end abruptly like the reigns of kings, but overlap and dovetail, the words *cataclysmal*, *uniformitarian*, and *evolutionary* may serve with some usefulness to emphasise the changes of outlook in the geology of the period under discussion.

Cataclysmal.—The nickname cataclysmal or catastrophic applies to those who saw no way of explaining the features of the earth's face—its ridges, wrinkles, dents, and scars—without postulating convulsions and cataclysms, fires and flood, not only on a scale vastly greater than any analogous occurrences now to be observed on our, on the whole, very sedate earth, but even different in kind. Cuvier, and to some extent Buffon, may be named as champions of the catastrophic theory.

Uniformitarian.—From this way of looking at things a recoil was inevitable when a growing appreciation of scientific method made it clear that in geological interpretation, as elsewhere, we must not

* The history of geology relied on is Karl Alfred von Zittel's *Geschichte der Geologie und Paläontologie*, 1899; translated (1901) by Dr. Maria Ogilvie-Gordon.

invent hypothetical agencies; that we must exhaust the full potency of known and verifiable causes before we admit even the need of postulating others which are unknown and unverifiable.

The uniformitarian view, well expressed by Hutton and Playfair, was right when it insisted that we must in our interpretation exhaust the possibilities of actually observable factors, but it was wrong if it assumed that these were necessarily all the factors, or that they had never changed in the rate or amount of their influence.

In the hands of Lyell (1797–1875) the uniformitarian interpretation found its best expression, and at the same time, as many think, signed its own death-warrant. For in spite of the progress of physics and astronomy since the time of Hutton, Lyell deliberately shut out the light of the evolution-idea—the thought of a beginning and of an end to the earth which the theory of energy presses home. "He consistently refused to extend his gaze beyond the rocks beneath his feet, and was thus led to do a serious injury to our science; he severed it from cosmogony, for which he entertained and expressed the most profound contempt, and from the mutilation thus inflicted geology is only at length making a slow and painful recovery." *

A reaction from extreme uniformitarianism was inevitable. It began to be felt that although "Lyell, in his great work, proved that the agents now in operation, working with the same activity as that which they exhibit at the present day, *might* produce the phenomena exhibited by the stratified rocks,

* W. J. Sollas, Pres. Address, Sec. C, *Rep. Brit. Ass.*, 1900; *Nature*, Sept. 13, 1900, p. 481.

. . . that is not the same thing as proving that they *did* so produce them." * Such proof can only be afforded by a detailed study of the strata, more extensive and intensive than even now exists.

But as this detailed study has proceeded, it has become more and more clear not only that the earth has evolved from a very different primitive state to its present form, but furthermore that through the immense expanse of its history there have been notable changes in the earth-sculpturing factors. The indisputable proof of great Ice-Ages and of enormous thrust-movements may serve to show that uniformitarianism recoiled too far from catastrophism. To try to explain the phenomena of glaciation without glaciers strained the uniformitarian theory to the breaking-point.

Evolutionary.—The cataclysmal geology was unscientific, for it invoked the aid of undemonstrable factors; the uniformitarian geology was inconsistent, for while it sought to interpret the past in terms of the present, it rejected the evolution idea which sums up the whole history as a process of becoming; the modern evolutionary geology has inherited the strength of the uniformitarian school and has given this fresh virility by recognising that the history of the earth is a natural development in which at every stage the present is the child of the past and the parent of the future. The evolutionist school differs from the uniformitarian, (*a*) in admitting in its fullest sense the hypothesis that the earth has had a natural history from a nebular or molten mass down to the twentieth century, and (*b*) in admitting the likelihood that in the course of the evolution there

* J. E. Marr, Address Section C, *Rep. Brit. Ass.*, 1896.

may have been rhythms and changes in the action of the known factors." *

SUMMARY.—"*From Steno onward the spirit of geology was catastrophic; from Hutton onward it grew increasingly uniformitarian; from the time of Darwin and Kelvin it has become evolutional.*" † "*The Catastrophists had it all their own way until the Uniformitarians got the upper hand, only to be in turn displaced by the Evolutionists.*" ‡

FOUNDATION STONES OF GEOLOGY.

Even in the later decades of the eighteenth century geology as a distinct science did not exist, but its sure foundations were being laid. Thus Sir Archibald Geikie has rescued from undeserved oblivion (in Britain at least) the name of Jean Étienne Guettard (1715–1786)—"the first to construct, however imperfectly, geological maps, the first to make known the existence of extinct volcanoes in Central France, and one of the first to see the value of organic remains as geological monuments, and to prepare detailed descriptions and figures of them. To him also are due some of the earliest luminous suggestions on the denudation of the land by the atmospheric and marine agents." **

Another illustrious pioneer was Nicholas Desmarest (1725–1815), who amid the labours of a life devoted to fostering the industries of France, found time to map the volcanic rocks of Auvergne, to work out a theory of the volcanic origin of basalt, to trace

* See J. E. Marr. Address Section C, *Rep. Brit. Ass.*, 1896, p. 775.

† Sollas, *loc. cit.*

‡ Geikie. *Founders of Geology*, 1897, p. 288.

** Sir Archibald Geikie. *Founders of Geology*, 1897, p. 46.

with persistent patience the various effects of denudation on beds of lava, to propound the doctrine of the origin of valleys by the erosive action of the streams which flow in them, and in short, to lay, not one but several of the foundation-stones of modern geology.

In Sir Archibald Geikie's fascinating account of the founders of geology, the next two names are Peter Simon Pallas (1741–1811) and Horace Benedict de Saussure (1740–1799). Pallas was in charge of a famous Russian expedition (1768–1774) ordered by the Empress Catherine II., primarily with the object of observing the Transit of Venus, but also with instructions to make a complete regional survey of everything from mountains to man. Geologically, the expedition was signalised by the discovery of the widespread remains of mammoth, rhinoceros, and buffalo in the Siberian basins, and by Pallas's researches on the origin and history of mountains. Far beyond the limits of geology, the work of Pallas has an acknowledged importance.

"The labours of De Saussure among the Alps mark an epoch, not only in the investigation of the history of the globe, but in the relations of civilised mankind to the mountains which diversify the surface of the land." He broke down a strange traditional prejudice against the horror of the great hills and inspired the modern enthusiasm for mountaineering; he began experiments in rock-making; he furnished a model of how mountain ranges should be studied and described; and he seems to have been the first to adopt the terms *Geology* and *Geologist.**

When theoretical critics came to Desmarest with objections, he used to say "Go and see"; and if it

* See Geikie, p. 88.

be true that any vindication of the necessity for an observational basis in science is now an anachronism, we should not forget the early struggles towards this essential virtue. Desmarest's conclusion as to the igneous origin of basalt may seem a small result for years of patience, but we have only to contrast it with the old idea that basaltic columns were petrified bamboo stems to see its historical importance. It may not be easy to cite any particular conclusion of De Saussure's which is now part of the framework of tektonic geology, but his lifework was none the less a vindication of the precept "Go and see."

Nowadays, no one who is interested in the nature and origin of the sculptured earth around him can "go and see" without bearing with him the idea that the earth's crust is a great history-book, that the various layers and strata are pages recording particular processes, and that there has been a "geological succession" still to be deciphered though he who runs may not read it. Yet this familiar and elementary idea of a geological succession had a long history!

Werner.—Sir Archibald Geikie refers to Lehmann, Fuchsel, and Werner as three observers who advanced the idea of geological succession during the latter half of the eighteenth century. Of the three, Werner was the most important. He tried to put minerals in order, as Linnæus had done for plants; he was one of the first to expound the general idea of the sequence of geological formations; and he was an influential teacher of great personal charm.

Hutton.—In 1785, after years of travel and thought, James Hutton communicated to the Royal Society of Edinburgh the first outlines of his *Theory of the Earth.*

For the main purpose of this volume, which is to illustrate the working of the scientific mood, the theory of the earth which Hutton suggested is full of significance. Significant, because its author had so clearly grasped the scientific method of seeking to appreciate the full force of known factors instead of invoking the aid of others whose reality is hypothetical. Waters wear the stones, the solid earth melts away, the mountain is transplanted piece-meal to the sea, there is a ceaseless decay of continents; on the other hand, underground forces cause upheaval, consolidated débris is once more brought to light, and molten masses are here and there thrust upward to form eruptive rock. What is, has been, and that through a vast antiquity of ages, so that "little causes, long continuing," have wrought great changes. The present is the child of the past and the parent of the future. In short it was the idea of *development* that Hutton had, perhaps subconsciously, in mind. The keynote of his work may be found in his sentence: "No powers are to be employed that are not natural to the globe, no action to be admitted of except those of which we know the principle, and no extraordinary events to be alleged to explain a common appearance." *

Unlike Werner, Hutton started from observations not from preconceptions. He studied the present, and in the process now occurring found the key to the history of the past. Among his conclusions we may note:—The aqueous origin of sedimentary rocks, the influence of subterranean force (essentially due to heat) in contorting strata, the theory of subterranean intrusions of molten matter forming veins or dykes

* *Theory of the Earth*, Vol. II. p. 547. Quoted by Sir A. Geikie, *Founders of Geology*, p. 182.

of "whinstone" and the like, the idea of the metamorphism of rocks under the influence of new conditions, and the doctrine of earth-sculpture by denudation (through rain, rivers, glaciers, etc.).

Neptunists and Plutonists.—The masterly and lucid *Illustrations of the Huttonian Theory* by Hutton's friend and disciple John Playfair, did much to help the new theory of the earth towards acceptance. But this was further delayed by the bitterness of the strange controversy which sprang up between Hutton's followers—nicknamed Plutonists—and those of Werner, who were similarly called Neptunists. Hutton had emphasised the importance of subterranean heat in consolidating and upheaving sedimentary deposits; Werner had almost exclusively emphasised the agency of water, believing that the rocks had arisen for the most part as precipitates in a primeval ocean. To one looking backward it does not seem an instructive controversy, and it is perhaps enough to say that the more stable doctrines of Hutton were those that survived.

Hall.—The Neptunists had urged against the Plutonists that if basalt and the like had really arisen from molten masses, they ought to be found as glasses or slags. To this Sir James Hall retorted by experiment, showing that basalt could be fused and vitrified, and that if a portion of this basalt-glass was re-fused and allowed to cool very slowly, it resumed its familiar stony textures. From pounded chalk, fused under pressure, he obtained a substance resembling marble. In another direction he also experimented most suggestively, for he arranged a mechanical device for contorting layers of clay (by lateral compression under considerable vertical pressure), and showed that the foldings of strata could thus be

imitated. These and other experiments may be justly regarded as the foundation of *experimental geology*.

William Smith.—While the Neptunists and Plutonists were bickering in Edinburgh—which has been a centre of geological activity through the century—the land-surveyor and engineer William Smith (1769–1839), was walking through the counties of England, and working out his momentous conclusion that the stratified rocks occur in definite sequence, and that each well-marked group can be recognised and tracked by its characteristic fossils. In 1815 he published his epoch-making Geological Map of England, and this he followed up during the succeeding nine years by twenty-one county maps, in the execution of which he was helped by his nephew and pupil, John Phillips. This was *the foundation of stratigraphical geology*.

In regard to the importance of William Smith's work, the verdict of one of the foremost living geologists may be cited. "No single discovery," says Sir Archibald Geikie, "has ever had a more momentous and far-reaching influence on the progress of a science than that law of organic succession which Smith established. At first it served merely to determine the order of the stratified rocks of England. But it soon proved to possess a world-wide value, for it was found to furnish the key to the structure of the whole stratified Crust of the earth. It showed that within that crust lie the chronicles of a long history of plant and animal life upon this planet, it supplied the means of arranging the materials for this history in true chronological sequence, and it thus opened out a magnificent vista through a vast series of ages, each marked by its own distinctive

types of organic life, which in proportion to their antiquity, departed more and more from the aspect of the living world." *

Along with the achievements of William Smith, we must place the researches of Cuvier and Brongniart, and of others who early realised the value of fossils as indices in determining the sequence of strata.

The idea of interpreting the history of the past in terms of changes observed in occurrence in the present; the conception of the sequence of strata; the recognition of the value of fossils as indices, are three of the foundation-stones of geology which were laid at the beginning of the nineteenth century.

THE EVOLUTION-IDEA IN GEOLOGY.

At various dates we find exceptional recognition of the Evolution-Idea as applied to the Earth. It fascinated a few long before Darwin brought it home to all. Thus Descartes propounded a scheme of the Earth's development from a globe of molten liquid, and Leibnitz's *Protogæa* (published long after his death, about the middle of the eighteenth century) contained a similar attempt. Buffon, too, starting with the bold idea that the Earth, like the planets, was detached from the mass of the sun by a cometary shock, sketched with a free hand the successive chapters of a problematical history in his *Epochs of Nature* (1778).

Even when uniformitarianism was in its full strength,—inquiring minds here and there were beginning to suspect that there was something to be said for the heresies of Buffon, Lamarck, Erasmus

* *Op. cit.*, 1892, pp. 9-10.

Darwin, and other pioneers who spoke of a progressive evolution of plants and animals. The evolution-idea was whispered by many, and a few proclaimed it prematurely on the house-tops.

The cosmological speculations of Kant and Laplace as to the possible evolution of suns and their systems did not apparently much excite the geologists, but they must have raised some disquieting thoughts. Sir William Thomson's early insistence (1862–1868) on the secular loss of heat from both earth and sun brought the question nearer home, for the conclusion was inevitable that the present state of affairs could not have lasted forever.

Without going back to a nebular mass we must at least think of a time when the earth was much hotter than now, when the waters of our ocean formed part of a hot atmosphere, and we may also look forward to a time when the earth will be much colder than now, and again without an ocean unless it be one of liquid air. In neither of these conditions could life, as we know it, exist. "Somewhere between these two indefinite points of time in the evolution of our planet it is our privilege to live, to investigate, to speculate concerning the antecedent and future conditions of things." * This is the evolutionist attitude.

It is interesting, however, to pause to notice a few of the lines of inquiry which led to the transition from Uniformitarian to what may be called Evolutionist geology.

From the early works of Fourier (1820), Poisson (1835), and Hopkins (1839), down to the more modern researches of Thomson and Tait and Helmholtz, there has been a prolonged attempt to map out the

* Sir John Murray, *Rep. Brit. Ass.*, 1899, p. 796.

great steps in the early history of the Earth before it became fit to be a home of life, and also to reach from physical and astronomical data some secure conclusion as to the present physical state of the Earth's interior.

Chapters in the Ancient History of the Earth.—The Earth probably had its beginning as one of the many rings swirled off from the great nebular mass which gradually condensed into our sun; but other origins are conceivable. In any case, it had a beginning as a rapidly rotating molten planet. It solidified about the centre into a metallic nucleus, which was probably composed in great part of iron; it was surrounded by a deep atmosphere, the larger part of which has been condensed into the waters of our present seas. Its molten ocean was profoundly disturbed by solar tides, for there was as yet no moon, and it was perhaps a particularly high tide which made the earth give birth to its satellite.

"This event may be regarded as marking the first critical period, or catastrophe if we please, in the history of our planet. The career of our satellite, after its escape from the earth, is not known till it attained a distance of nine terrestrial radii; after this its progress can be clearly followed. At the eventful time of parturition the earth was rotating, with a period of from two to four hours, about an axis inclined at some 11° or 12° to the ecliptic. The time which has elapsed since the moon occupied a position nine terrestrial radii distant from the earth is at least fifty-six to fifty-seven millions of years, but may have been much more." *

"The outer envelope of the earth drawn off to form the moon was charged with steam and other

* W. J. Sollas, Pres. Address, Sec. C, *Rep. Brit. Ass.*, 1900; *Nature*, 13th Sept., 1900, p. 482.

gases under a pressure of 5,000 lbs. to the square inch; but as the satellite wandered away from the parent planet this pressure continuously diminished. Under these circumstances the moon would become as explosive as a charged bomb, steam would burst forth from numberless volcanoes, and while the face of the moon might thus have acquired its existing features, the ejected material might possibly have been shot so far away from its origin as to have acquired an independent orbit," * and some of the meteorites which now descend upon the earth may be returned portions of the early envelope.

Soon after the birth of the moon, the earth became consolidated (with a surface temperature of about 1170°C.), and the moon may have been influential in determining high-pressure areas where the crust was depressed, and low-pressure areas where it was lowered. This, as Sollas says, was the second critical period in the history of the earth, the stage of the "consistentior status." Since this epoch, on Lord Kelvin's estimate, twenty to forty millions of years may have elapsed.

Below the surface the temperature increased, as it still does; at a depth of twenty-five miles, it would be (according to Lord Kelvin's calculations) about 1430°C., or 260°C. above the fusion point of the matter forming the crust. But the great pressure at this depth would counteract the heightened temperature, and still keep the crust solidified even at a depth of twenty-five miles.

When, with continued cooling, the temperature of the surface fell to 370°C., the steam in the atmosphere would begin to liquefy, and this was the first step in the origin of the oceans. Supposing, as

* Sollas, *loc. cit.*

Sollas suggests, a localisation of the water in primitive faint depressions (anti-cyclonic areas), and a corresponding reduction of pressure on the incipient continental areas, there might result an expansion of the underlying rock of these areas, "for a great change of volume occurs when the material of igneous rocks passes from the crystalline state to that of glass." In some such way, the ocean basins might be deepened and the continental areas raised. The hot water of the primeval ocean would act energetically on the silicates of the primitive crust; it would begin to be "salt" with saline solutions and to precipitate deposits. Since the condensation of the oceans, Prof. Joly suggests a lapse of eighty to ninety millions of years.

To sum up dogmatically would be absurd, but it may be said that a nebular mass probably gave rise to a rapidly rotating molten planet; that after central solidification, this may have given birth to the moon; and that as cooling slowly continued, there followed the consolidation of the crust and the beginning of the distinction between ocean basins and continental areas.

Through phases more or less like those outlined above the Earth has reached its present state. The vast nucleus or "centrosphere" is practically solid, the melting-point of the metals and metalloids being raised by the immense pressure. Outside the central mass there is "a shell of materials bordering upon fusion," that which Sir John Murray calls the "tektosphere." On this plastic shell there rests the heterogeneous and wrinkled crust or lithosphere, always slightly pulsating.

Wrinkling of the Lithosphere.—How the crust or lithosphere has come to be elevated into continental

areas, on an average three miles above the ocean floor and to be folded into mountain chains, is one of the most difficult of geological problems, but there are several factors on which the evolutionary geologist relies. Perhaps the most important is the contraction of the centrosphere. But, before noting a few opinions of experts on this subject, it may be useful to recall that, stupendous as mountain-chains are, their height is minute when compared with the radius of the earth. Indeed, it has been pointed out that on an artificial globe a foot in diameter, they should not stand out more than the slight elevations which might result where the edges of the covering paper-slips overlap.

"As the solid centrosphere slowly contracted from loss of heat, the primitive lithosphere, in accommodating itself—through changes in the tektosphere—to the shrinking nucleus, would be buckled, warped, and thrown into ridges. . . . The compression of mountain chains has most probably been brought about in this manner, but the same cannot be said of the elevation of plateaus, of mountain platforms, and of continents." *

"It was at first imagined that during the flow of time the interior of the earth lost so much heat, and suffered so much contraction in consequence, that the exterior in adapting itself to the shrunken body, was compelled to fit it like a wrinkled garment. This theory, indeed, enjoyed a happy existence till it fell into the hands of mathematicians, when it fared very badly, and now lies in a pitiable condition, neglected of its friends." † The mathematicians maintained

* Sir John Murray, *Rep. Brit. Ass.*, 1899, p. 797.

† Sollas, *Rep. Brit. Ass.*, 1900. See *Nature*, Sept. 13, 1900, p. 487.

that the amount of contraction was altogether inadequate to explain the wrinkling, but Prof. Sollas finds sufficient flaws in the data to warrant him in still maintaining the theory of contraction. "The contraction of the interior of the earth, consequent on its loss of heat, causes the crust to fall upon it in folds, which rise over the continents and sink under the oceans, and the flexure of the area of sedimentation is partly a consequence of this folding, partly of overloading." *

Another factor may be chiefly alluded to. Since the floor of the ocean has a temperature about zero, and is some three miles below the continental level, surfaces of equal internal temperature will not be spherical, but will rise beneath the continents and sink beneath the ocean, and the effect will be to render the continents mobile as regards the ocean floor; or *vice versâ* (Sollas).

We have cited enough to illustrate a kind of inquiry eminently characteristic of the end of the nineteenth century which the new century is certain to develop to more stable and precise results.

The general result may be summed up in a sentence; the contraction of the interior probably accounts for much of the folding and crumpling of the exterior; other physical factors are and have been at work; and the transforming influences of water, of the atmosphere, and of life have been continuous and momentous since they first began to act.

It must not be supposed that the evolution-idea in Geology has been restricted in application to the recondite problem of the Earth's early phases; the idea has influenced the whole science and is illustrated in the modern treatment of river-development, or of coral reefs, or of details of scenery, and so on,

* Sollas, *loc. cit.*

just as markedly as in connection with the big question of the history of the Earth as a whole.

AGE OF THE EARTH.

In the early days of geological science, the prevalent opinion seems to have been that the earth was about 6,000 years old. But this belief was for the most part an outcome of "wresting the Scriptures" from their proper use, and is quite irrelevant in scientific discussion.

The Age of the Earth as Realised by Uniformitarians.—When James Hutton (1726–1797) began to show that the present supplies the key to the interpretation of the past, and saw "the ruins of an older world in the present structure of the globe," it became plain to inquiring minds that the earth must be old beyond all telling.

William Smith's revelation of the succession of strata in England—the vision of age before age stretching back into an inconceivably distant past; the founding of palæontology by Cuvier and others, and the suggestion of successive faunas and floras leading us back and back to the mist of life's beginnings; the publication of John Playfair's *Illustrations of the Huttonian Theory* (1802); and other great events led to an accentuation of the idea of antiquity. Indeed, Playfair went so far as to deny that either earth or cosmos furnished tangible hint of any beginning at all. Thus the earth, which had not long before been credited with a short duration of 6,000 years, was at the beginning of the century conceived of as a sort of inanimate Methuselah, "without beginning of days or end of years."

Recognitions of Limits.—A reaction began in 1862, when Lord Kelvin (then Sir William Thomson) sent

his first shell into the camp of the geologists, which he has not since ceased to bombard. From that date the history has been this,—the physicists have calculated out certain limits; the geologists have agreed that they do not require eternity, but yet much more than the physicists will grant them; there has been much criticism of data and calculations and some reconsideration on both sides; of late the biologists have also insisted on being heard.

(*a*) *Physical Arguments.*—The chief arguments of the physicists as to the age of the earth are based (1) on the downward increase of terrestrial temperature, (2) on the retardation of the earth's angular velocity by tidal friction, and (3) on the limitation of the sun's age. Lord Kelvin began by declaring that the age of the earth must be more than twenty millions of years, and less than four hundred millions; but he subsequently cut down his maximum to the former minimum, and Professor Tait would not allow even half as much. In one of his last utterances on the subject, Lord Kelvin states " it was more than twenty and less than forty million years, and probably much nearer twenty than forty." *

That the physicists are far from being agreed among themselves may be inferred from the frank statement of Professor George Darwin: "*At present our knowledge of a definite limit to geological time has so little precision that we should do wrong to summarily reject any theories which appear to demand longer periods of time than those which now appear allowable.*" †

(*b*) *Geological Arguments: From the rate of deposition of rock-forming materials.*—Ever since Hutton published his observations and reflections on

* Pres. Address Victoria Institute for 1897. Phil. Mag., January, 1899.

† *Rep. Brit. Ass.*, 1896, p. 518.

the decay of continents, it has been a recognised fact that there is a universal degradation of the dry land. The span of the longest human life is but a tick of the geological clock, and so we speak of the eternal hills. But there is no doubt in the mind of any observer that even the hills are slowly melting and crumbling away. "The hills are shadows, and they flow from form to form, and nothing stands." Rain and frost, lichens and burrowing animals, running water and whistling wind, and other agencies contribute to the unceasing weathering and denudation. There are, indeed, conservative agencies, but the wasting goes on steadily. The present land surface is being reduced in height, on an average of $\frac{1}{2400}$ to $\frac{1}{3000}$ foot per annum. But what is lost here is gained somewhere else, denudation and deposition must be almost equivalent in amount (though not in area, the latter being usually much smaller), and thus we can arrive at some estimate of the amount of wasting by measuring the amount of sediment deposited. "Actual measurement of the proportion of sediment in river water shows that while in some cases the lowering of the surface may be as much as $\frac{1}{730}$ of a foot in a year, in others it falls as low as $\frac{1}{6800}$. In other words, the rate of deposition of new sedimentary formations, over an area of sea-floor equivalent to that which has yielded the sediment, may vary from one foot in 730 years to one foot in 6,800 years." *

Now, a considerable part of the outer crust of the earth is made up of sedimentary rocks; among these it is possible with considerable accuracy to distinguish the deposits which were laid down at different

* Sir Archibald Geikie, Pres. Address, *Report Brit. Ass.* for 1892, p. 21.

and successive times (as proved in *some* cases decisively by their fossils and in other cases by other facts); and "on a reasonable computation, these stratified masses, where most fully developed, attain a thickness of not less than 100,000 feet." * *Therefore, if* we assume that the present rate of change is at all comparable to the past rate of change, we can form geologically some estimate of the antiquity of our earth. "If they were all laid down at the most rapid recorded rate of denudation, they would require a period of seventy-three millions of years for their completion. If they were laid down at the slowest rate they would demand a period of not less than six hundred and eighty millions." †

But how much experts may differ is here again illustrated, for Prof. Sollas says:—"The total maximum thickness of the stratified rocks is 265,000 feet, and consequently if they accumulated at the rate of one foot in a century, as evidence seems to suggest, more than twenty-six millions of years must have elapsed during their formation." ‡

Against this line of argument various objections may be raised. It may be said that the rate of denudation and therefore of deposition may have been much more rapid a few million years ago than it now is, and the possibility cannot be denied. But some evidence should be forthcoming; and there is not much. In ancient sedimentary rocks we see ripple marks and sun-cracks and worm or mollusc tracks and it may even be the markings of desiccated jellyfishes, just as we see them on the beach to-day, and this certainly does not point to rapid deposition.

* A. Geikie, *op. cit.*, p. 21.
† A. Geikie, *op. cit.*, p. 21.
‡ W. J. Sollas, Address Section C, *Rep. Brit. Ass.*, 1900, *Nature*, Sept. 13, 1900, p 485.

Moreover, we must recall the fact that the sedimentary rocks are in scores of cases interrupted in a manner which forces us to infer periods of upheaval or subsidence or volcanic intrusion,—still further extending the demand for millions of years.

In an exceedingly interesting paper, Goodchild * has tried to estimate the time required for the various sedimentary formations considered seriatim, and the time represented by great unconformities, and computes the total time since the commencement of the Cambrian period at over 700,000,000 years. But life was already ancient in the Cambrian times, and this leads, as Goodchild indicates, to an enormous increase of the seven hundred millions.

Argument from the Saltness of the Sea.—Another interesting line of argument is that which has led Prof. Joly to conclude that eighty to ninety millions of years represent the maximum period of time since the oceans were formed. His argument is that since the salt sea was once fresh, and since the saltness is due to dissolved salts carried into the sea by rivers, an estimate of the annual amount brought down by the rivers will show how long it must have taken to give the sea its present salinity. Taking sodium alone, it is computed that the amount in the sea is at least ninety millions of times greater than the quantity which rivers pour in annually (about 160,000,000 tons). Joly's argument is clear and simple; everything depends, however, on the reliability of the data.

(*c*) *Biological Arguments.*—Apart from domestication and cultivation we know almost nothing in regard to the present rate of variation of living creatures, though researches like those of Prof. Weldon

* *Proc. Roy. Phys. Soc.*, Edinburgh, xiii., 1897, pp. 259–308.

on the crabs of Plymouth Harbour are beginning to remedy this discreditable ignorance. Until we have much information of this sort it is quite idle for one biologist to say that he thinks one hundred millions of years enough for the evolution of living creatures, and for another to declare himself contented with a grant of a quarter of that amount.

We are certain that the evolution of backboned animals, from Silurian Fishes to Man, has occupied "a period represented by a thickness of 34 miles of sediment"; and although we are familiar with long-lived types, like the tongue-shell, *Lingula,* which has persisted with "next to no perceptible change" from the Cambrian till to-day, we are also aware of races, like some of the extinct Reptiles, which have appeared, grown great, and disappeared within a relatively short time, as time goes. "To select *Lingula,* or other species equally sluggish, as the sole measure of the rate of biologic change would seem as strange a proceeding as to confound the swiftness of a river with the stagnation of the pools that lie beside its banks" (Sollas).

The biological argument has been particularly discussed by Professor Poulton,* with the general result that he feels it necessary to demand much more than even the geologist demands. The general fact of importance is that in the oldest fossil-containing rocks we find highly specialised animals which must have had a long history behind them; that in the Cambrian, Ordovician, and Silurian almost all the great phyla or stocks of animals are already represented, and in many cases by forms which are anything but primitive. To the geologist's computation of the period required to account for the strata between the

* Address Section D, *Rep. Brit. Ass.*, 1896, pp. 808–828.

Cambrian and those now forming, we are forced to make a large addition in order to account for the evolution of the rich Cambrian fauna.

Under the Cambrian beds there is evidence of some 80,000 feet of stratified rock, in which there are no remains of organisms, but during which it seems almost necessary to assume that the chief types of backboneless animals and simple plants had their origin. The absence of fossils is most plausibly interpreted as mainly due to the absence of hard or preservable parts in the primitive forms; and even the modest estimate of twenty-six millions of years as the period, since the earth became fit to be a home of life, leaves a considerable number of millions for this pre-Cambrian period during which the unicellular creatures may have given origin to multicellular bodies, taking the form of polyps and worms, even of trilobites and molluscs. The suggestion has often been made that in early times, among simple creatures, the rate of progress may have been much more rapid than among the higher forms whose stages of evolution are recorded in the rocks. But this is mere opinion.

At the beginning of the nineteenth century there was an irrelevant belief that the habitable earth was some 6,000 years old. But the work of James Hutton alone was enough to convince the unprejudiced that the antiquity of the earth must be inconceivably great. The tendency of progressive geologists to draw without stint upon the bank of time, had to face a wholesome reminder from the physicists that their credit was not unlimited. The limitations imposed by the physicists have been vigorously rebelled against, and criticism has tended to show that they were too narrow and not altogether warrantable. The

data as to the rate of cooling of earth and sun, as to tidal retardation, as to the rate of sedimentation, as to the rate of evolutionary change in organisms, are in varying degrees only approximate, and the age of the earth remains a problem for the twentieth century.

READING THE ROCK-RECORD.

We have now grown accustomed to the idea that the strata of the earth's crust form a great library of historical documents relating to the history of our world and its inhabitants,—a library never very complete, but, worse than that, disordered, half-burnt, flooded, and buried.

There are two ways of reading history in this underground library. The nature of the rock, sandstone or shale, limestone or chert, or otherwise—tells the experienced observer something about the physical conditions of the time when the rock was formed; and the relation of one stratum or set of strata to another makes it possible to determine the order of succession in time. Yet, on the whole, the decisive evidence as to the physical conditions of the distant age and as to the order of succession in time is afforded by the remains of plants and animals which the rocks contain.

That fossils furnish the clue which makes it possible to determine the historical order of sequence in the various strata that compose the earth's crust is a familiar fact now; but the realisation of it was a momentous event in the history of geology. And it may be noted that although the study of fossils had begun in the seventeenth century in the inquiries of Stenson, Hooke, Woodward, and others, almost no progress was made till the end of the eight-

eenth when in 1795 Cuvier and Brongniart began their immortal researches on the remains of animals and plants in the Paris basin, and William Smith (1799) published his table of strata and their characteristic fossils. It may thus be said that the utilisation of fossils as aids in stratigraphical geology is only about a century old. But the whole progress of the century may be illustrated by the difference between Smith's general use of fossils and—say Lapworth's specific use of Graptolites in determining the succession of closely approximated zones.

Gradually the key which Smith has used to so much purpose came to be generally appreciated. Zittel notes the historical importance of the *Outlines of the Geology of England and Wales*, by W. D. Conybeare and W. Philips (1822) in which the indispensable value of fossils was clearly recognised. Lyell, Deshayes, d'Omallius d'Halloy and Bronn are probably the most outstanding of the early geologists who vindicated the union of palæontology and geology which has proved so profitable to both sciences.

To follow the development of stratigraphical geology from Sir Roderick Murchison (1792–1871) and Professor Adam Sedgwick (1785–1873) onwards through the century is far beyond the scope of this sketch. As with comparative anatomy, the results of stratigraphical geology are necessarily for the most part quantitative and appeal more to the expert than to the general reader. It may be said, however, that

"While the whole science of geology has made gigantic advances during the nineteenth century, by far the most astonishing progress has sprung from the recogni-

tion of the value of fossils. To that source may be traced the prodigious development of stratigraphy over the whole world, the power of working out the geological history of a country, and of comparing it with the history of other countries, the possibility of tracing the synchronism and the sequence of the earth's surface since life first appeared upon the planet." *

PROBLEMS OF EARTH-SCULPTURE.

What is often called "Dynamical Geology" is concerned with the factors which have wrought out the present state of the various land-forms. It has to do with the evolution of scenery, or with earth-sculpture,—one of the most fascinating problems of geology.

Air, water, ice, volcanoes, earthquakes, changes in coast-level, thrust-movements, living creatures, are the most important factors in the process by which the face of the earth has been and is being slowly changed. To some of these we wish to refer in this section, while others have found notice elsewhere.

Hutton's Recognition of Factors in Earth-Sculpture.—In his *Theory of the Earth* (1788), Hutton recognised the following factors as operative in changing the earth's surface:—degradation of land by atmospheric and aqueous agencies, deposition of the débris as sediment in the ocean, consolidation and metamorphosis of sedimentary deposits by the internal heat and by injection of molten rock, disturbance and upheaval of oceanic deposits, and formation of rocks by the consolidation of molten material both at the surface and in the interior of the earth.

* Sir A. Geikie. *Founders of Geology*, 1897, p. 241.

When this is compared with a recent book on Physical Geology, such as Prof. James Geikie's *Earth Sculpture,* we are at once impressed by the fact that only a few additional modes of operation have been discovered in the course of the century. The progress has been in measuring the efficacy of the factors which Hutton recognised, rather than in discovering new ones.

A Case of Probable Uniformity.—It is a familiar fact that water and air in various ways denude the solid land, sometimes acting chemically, as in the breaking up of silicates into insoluble and soluble constituents, sometimes acting more mechanically in disintegrating without decomposing. The insoluble results of denudation are deposited as gravel, sand, and mud; the soluble constituents may also be deposited (by evaporation, chemical action, or through the agency of living creatures) to form carbonates, sulphates, chlorides, or less frequently oxides. This is a world-wide process, which probably went on in pre-Cambrian times very much as it does to-day. "There is no evidence," says Prof. J. J. H. Teall (now Director-General of the Geological Survey of Britain), "that any of our sedimentary rocks carry us back to a time when the physical conditions of the planet were materially different from those which now exist." *

Study of Volcanoes.—The acrimonious controversy between "Vulcanists" and "Neptunists," which has been already referred to, dragged its weary length into the first quarter of the nineteenth century. The "Vulcanists," championed by Hutton, upheld the igneous origin of such rocks as basalt; the "Neptunists," led by Werner, declared igneous

* Address Section C, *Rep. Brit. Ass.* for 1893, p. 737.

rocks to be chemical precipitates in water; and Werner went the length of maintaining that volcanic action was altogether a modern phenomenon.

There was more progress in the work of Alexander von Humboldt (published 1808–1823) who took a world-wide survey of volcanoes, and concluded from their distribution that they could not be due to merely local causes (like coal-pits on fire), but must be interpreted in reference to the state of the earth's interior and clefts in the overlying crust. Humboldt's position was strengthened by the work of his friend Leopold von Buch, who began as a Neptunist, but was soon led by observation in many countries to sounder views. Relying, like Hutton, on the expansive power of the internal heat of the earth, he made a point of distinguishing from true volcanoes what he called "craters of elevation." These he supposed to be due to huge blister-like elevations of the strata of the crust, which eventually collapsed, though without actual volcanic eruption.

In 1825, George Poulett-Scrope published the first edition of his classic book on volcanoes, in which he gave a careful description of the physical facts, and sought to explain volcanic action both past and present on a simple hypothesis. Supposing that subterranean rock-masses were saturated with water, and that this became heated from the interior, the expansive force of the steam would account for eruptions. Like Lyell (1830), he entirely opposed von Buch's theory of "craters of elevation" as contrasted with eruptive volcanoes.

For many years a healthy conflict of opinions continued between supporters of von Buch, such as Daubeny, Elie de Beaumont, and Dufrenoy, and

supporters of Poulett-Scrope, such as Prévost, Hoffmann, and Montlosier.

Facts were industriously gathered on both sides, splendid work was done by both schools, but after Lyell's study of the Canary Islands and Madeira in 1854, and Poulett-Scrope's papers in 1856 and 1859, von Buch's theory began slowly to give way. Sir Archibald Geikie's work on *The Ancient Volcanoes of Great Britain* (1897) may be mentioned as a splendid illustration of the achievements of modern volcanology.

Causes.—The description of active and extinct volcanoes has reached a high degree of perfection; much has been done in interpreting existing features of the earth in terms of ancient volcanic activity; chemists and petrographers have contributed greatly to our knowledge of volcanic products; but in regard to the causes of volcanic action there seems still considerable uncertainty.

Standing by itself is the theory of Mallet, that thrusts in the crust (due to cooling of the interior) may have locally crushed rocks to powder, thus developing great heat—sufficient to melt the rock. But proof of the crushing to powder and of subsequent melting seems absent. "This hypothesis, attractive as it may be at first sight, appears to be destitute of any real foundation." *

A survey of distribution of volcanoes is of some assistance. "It appears to lead to two inferences—one that volcanoes are commonly arranged in lines; the other, that when active they are generally in the neighbourhood of large sheets of water. The former fact suggests a connection between volcanic

* Prof. T. G. Bonney. *The Story of our Planet*, London, 1893, p. 287.

vents and lines of weakness or fracture in the earth's crust; the latter that their paroxysmal activity, perhaps even their existence, depends upon the proximity of water, so that 'without water no eruption' might almost be regarded as an axiom." *

On the other hand, it seems unsafe to lay too heavy a burden on the expansive force of steam, for though steam is invariably present in volcanic discharges, its amount often appears (as in Hawaii) disproportionate to the work done.

"The most probable view is that volcanoes are closely related to those earth movements which have resulted in the flexing and fracturing of strata. All the greater wrinkles of the earth's surface—its ocean-basins, continental plateaus, and mountains of elevation—owe their origin to the sinking-in of the crust upon the cooling and contracting nucleus. The crust yields to the enormous tangential pressure by cracking across and wrinkling up, in various linear directions, and it is along these lines of fracture and flexure that molten matter and heated vapours and gases are enabled to make their escape to the surface. So far, then, geologists are agreed as to the close relation that obtains between fracturing, folding, and volcanic action. But beyond this agreement ceases." †

Study of Earthquakes.—In the first half of the nineteenth century most geologists seem to have accepted the conclusion of Humboldt (1815), that earthquakes were closely associated with volcanic action.

A long observational period in which data as to earthquakes were accumulated by many workers, such as Alexis Perrey in Dijon, de Rossi in Italy,

* Bonney, *op. cit.*, p. 283.

† Prof. James Geikie. Article, *Volcanoes*, *Chambers's Encyclopædia*.

and R. and J. W. Mallet in England, was not marked by any general conclusion of importance.

In 1873 and 1874, Suess changed the current of opinion by showing that earthquakes recurred in definite lines determined by the structure of the crust, and quite independently of volcanic action.

A by-path was opened up by Perrey's theory, suggested by his statistical data, that the attraction of the moon caused what may be called internal tides of the glowing internal fluid mass of the earth's interior, and that these, rising at times against weaker parts of the heterogeneous unequal crust, caused earthquakes. A somewhat similar tidal theory was elaborated by Rudolf Falb, partly on astronomical grounds, and led him into the dangerous field of prophecy. Against both theories it seems sufficient to urge the enormous probability in favour of the view that the nucleus of the earth is solid.

The general inclination at present seems to be towards a combination of the conclusions of Humboldt and of Suess. On the one hand, earthquakes may be associated with volcanic activity,—subterranean explosions of gases, the pressure of subterranean flows of lava, the collapse of unsupported strata, may set up undulations in the crust. On the other hand, even when volcanoes and earthquakes occur together in the same country, it has been shown that there may be no demonstrable connection between them. This has been especially well brought out by Prof. J. Milne's seismological work in Japan. He remarks that "earthquakes generally occur in mountainous countries where the mountains are geologically young, or in countries where

there is evidence of slow secular movements like elevation. These latter movements are usually well marked in volcanic countries, and it is not unlikely that the majority of earthquakes, even in volcanic countries, are the result of the sudden yielding of rocky masses which have been bent till they have reached a limit of elasticity. The after-shocks are suggestive of the settling of disjointed strata." *

It is probable, then, that while some earthquakes are due to subterranean explosions of steam or other volcanic disturbances, the majority are due to slips or fractures of the earth's crust in areas of great strain.

The improvement in the delicacy of earthquake-measuring instruments (seismometers) has led to a great extension of our knowledge in regard to the diffusion of the undulations, and to a recognition of the frequent minor tremors which would otherwise have remained undetected.

Crust-Movements.—It was in Scandinavia that careful attention was first paid to those secular changes of upheaval and depression, which, notwithstanding their slowness, are more important geologically than either earthquakes or volcanoes. The facts are particularly clear along the Scandinavian coast, and even the fisher folk could not but be impressed when they saw that the lines once cut to mark sea-level became gradually more and more inaccurate. Indeed the rise of land in Northern Sweden has been estimated at as much as 2½ feet in a century.

From Scandinavia the study of raised beaches and uplifted shell-beds spread to Britain, and all over the world. Evidences of depression were also

* *Rep. Brit. Ass.* for 1892, p. 128.

found in submerged forests and even villages. Proofs of the gradualness of these changes prevailed against theories of sudden oscillations. Almost all the eminent geologists of the century have contributed to the subject.

While the prevailing interpretation has always been that the local level of the land changed while that of the sea remained constant, there have been many who have insisted that the sea-level may also change,—in consequence of great subsidences, accumulations of sediment, formation of polar ice-caps, and so on.

The complications of the problem and the difficulties in the face of any general theory are recognised in the splendid work of Suess (*Antlitz der Erde*) which touches the high-water mark in this department of geology.

Mountain-Making.—Far ahead of his time, Steno, in 1669, tried to interpret the hills and valleys of Tuscany in terms of the collapse of the earth's crust, the uplift of stratified rocks, and the accumulation of volcanic material. Long afterwards, Hutton found satisfaction in *referring* elevations of the crust to the expansive power of the subterranean heat, to which volcanoes acted as safety valves. Leopold von Buch and Poulett-Scrope were among those who upheld Hutton's theory, and sought to improve upon it. From 1829 to 1852 Elie de Beaumont illustrated the important idea that the gradual cooling of the earth led to the crumbling of the crust. James Hall in 1859 pointed out that the gradual accumulation of sedimentary masses in areas of depression may be associated with a corresponding elevation of mountain chains elsewhere. Dana returned to the consideration of the effects produced

on the crust by the contraction of the nucleus, and studied these with deeper analysis than heretofore, laying special emphasis on the horizontal lateral pressures involved in the shrinkage. N. S. Shaler in 1866 had used the contraction theory to explain the origin of continents as well as mountain chains, and Le Conte was also closely associated with Dana's work.

A new chapter begins with the work of Edouard Suess. "A small book, published in 1875 under the title, *The Origin of the Alps,* contained in clear-cut outlines a wealth of new ideas; it came like vivifying rain on the dry ground." * This was a preliminary suggestion of the author's famous *Antlitz der Erde* (1897). In the preface to the French translation of this geological masterpiece, Marcel Bertrand says:—"The creation of a science, like that of a world, demands more than a day; but when our successors come to write the history of our science, they will say, I am persuaded, that the work of Suess marks the end of the first day, when light first shone."

No one could give a summary of Gegenbaur's *Comparative Anatomy,* and yet it is one of the zoological milestones. The same must be said in regard to the work of Suess. It is a comparative anatomy and comparative embryology of land-forms, unified by an evolutionary idea; but how can it be summarised?

The theory that continents or mountains are due simply to a force working from below upwards is an unworkable crudity, though it must be allowed that the shrinkage of the crust from contraction of the nucleus caused vertical as well as horizontal dislo-

* Zittel, p. 462.

cations, since it induces radial and tangential strains. The theory that volcanic eruptions count for much in mountain-making is a superficial exaggeration. The architecture (Tektonik) of mountains must be studied in detail. They have a one-sided structure—in the Alps, the Balkan, the Caucasus, and Ararat—all expressions of a tangential force from south to north in Europe, and towards the south in Asia. But besides the dislocations of the lithosphere there have been great transgressions and regressions of the hydrosphere, not less momentous than the rise of mountain chains. The continents, as Shaler said, are due to contractions of the whole crust, while mountains are due to foldings of the outer layers in consequence of contractions in the deeper. But, just as in pack ice, there may be unyielding masses, which have to be piled one upon the other, or may be simply undisturbed and overlapped.

RECOGNITION OF ICE AGES.

Evidences of Glaciation.—In a suitable area, such as Scotland, every beginner in geological study is familiar with the smoothed contours of rocks, the striated surfaces, the "crags and tails," the boulder-clay and so on, which prove the former presence of enormous glaciers, and that at no very distant date. Many of the phenomena are obvious and they were of course familiar to Hutton and his friends. But they received other interpretations than that which seems to us almost self-evident—now that the riddle has been read. They were explained as due to floods of water and strong tides, and these were again explained by supposing elevations or depressions wherever they were required.

Study of Glaciers.—The study of glacial action may fairly date from H. B. Saussure's famous *Travels in the Alps,* in which glaciers and moraines were described with detailed accuracy. Saussure was followed by Hugi, an enthusiastic mountaineer, who explored the upper reaches and was the first literally to sojourn on the slowly moving ice-sheets. An important step was taken by Venetz, an engineer, who, from 1821 onwards, sought to prove from the distribution of moraines the enormous prehistoric development of glaciers, not only in Switzerland, but in North Europe. Venetz converted J. v. Charpentier, who, in turn, strengthened his friend's argument with evidence drawn from the wide occurrence of erratic blocks which only ice could have carried.

Agassiz.—Louis Agassiz soon caught the enthusiasm, and began along with Charpentier and the botanist Schimper a prolonged series of excursions and observations which led him to the conception of a Great Ice Age, which was developed in a book published in 1840. From his study of past floras and faunas Schimper had been led to the idea of alternating periods of desolation and rejuvenation as a Great Ice Age.

Agassiz was stronger in his description of glacial phenomena and in his recognition of the previously wide extension of glaciers (as proved by erratic blocks, striated surfaces, etc.) than in his Ice Age theory. But let us try to summarise his conclusions. Before the elevation of the Alps, an immense ice-sheet covered most of the northern hemisphere; the Alps arose, and the débris of broken ice-sheet and shattered strata fell on the adjacent glaciers, which bore off their heavy burden, grinding the movable

rocks beneath them to powder, striating and polishing the immovable; but when the Alps had been upheaved, the surface of the earth was warmed anew, the ice melted, erosion valleys were formed, erratic blocks were left stranded, and so on.

Along with much truth, there was also much fancy and exaggeration in this theory, and the unwholesome taint of catastrophism was especially distinct in his assumption of successive ages of low temperature at the close of the various geological periods.

Charpentier's *Essai sur les Glaciers* (1841) was more thoroughly scientific than the work of Agassiz. Von Zittel speaks of its precision—recalling that of de Saussure, of its thoroughness, of its basis in original observations. He questioned Agassiz's theory of one great northern ice-sheet, older than the Alps, but pictured rather a great extension of presently existing glaciers,—thus reacting to an opposite extreme. In subsequent works, Agassiz modified some of his views in deference to Charpentier, and as the result of his own extended experience in Scotland and in America.

According to Agassiz the Swiss glaciers must once have been large enough to reach to the Jura,—a conclusion that seemed to many of his contemporaries an incredible extravagance. As Sir Archibald Geikie notes, "even a cautious thinker like Lyell saw less difficulty in sinking the whole of Central Europe under the sea, and covering the waters with floating icebergs." . . . "Men shut their eyes to the meaning of the unquestionable fact that, while there was absolutely no evidence for a marine submergence, the former track of the glaciers could be followed mile after mile, by the rocks they had scored and the blocks they had dropped, all the way from their

present ends to the far-distant crests of the Jura." * In fact the proof might be taken as a model of scientific inference.

The Drift Theory.—In spite of the conclusive researches of Agassiz and Charpentier, equally able men refused to be convinced. Thus Leopold von Buch and many adherents delayed the recognition of the ancient glaciers by a theory of great floods, supposed to have borne Northern blocks even to the foot of the Alps. On the other hand, the polar experiences of Parry, Scoresby, and Ross led some British geologists—Lyell, De la Beche, Charles Darwin, and Roderick Murchison—to a "drift-theory," which supposed the transport of erratic material by icebergs, and in this they were supported by Böthlingk, Bronn, Forchhammer, Frapolli, and others.† The influence of this "drift-theory"—which seems a big error enclosing a fragment of truth—was considerable, and lasted till 1879 when Penck had the satisfaction of giving a merciful death-blow to a theory which was slowly dying of inanition.

It would require a great expert to select wisely from the succession of events, but perhaps we may associate the next great step with Andrew Crombie Ramsay who made a profound study of the glaciation of Scotland and Wales (1854), detected traces of at least two ice ages, and inferred the existence of glaciers in the Permian. This revived the idea of recurrent ice ages. Very important, also, were the observations on the existing glaciers of Greenland from those of Rink (1857) to those of Torell (1872), and onwards to those of Nansen.

That evidences of glaciation were obvious in countries now free from glaciers, that there had been

* *Founders of Geology*, p. 273. † Zittel, *op. cit.*, p. 342.

a relatively recent Great Ice Age, probably interrupted by mild periods, and that there had been glacial action even in geological antiquity, were gradually accepted as well-established conclusions. There sprang up, however, a memorable controversy as to the part glaciers had played in gouging out Alpine lakes, valleys, and fiords. To some it seemed that this erosive action which Gabriel de Mortillet (1858) was one of the first to expound was a certainty; to others, such as Heim, glaciers were regarded rather as conservative than as destructive agents. Modern opinion has inclined strongly, though not unanimously, in favour of the theory that glacial erosion has been a very important sculpturing factor.

Professor James Geikie's *Great Ice Age* may be mentioned as a crowning work of the nineteenth century study of glaciation, as a modern critical development of the work of Agassiz and Charpentier, and as a fascinating contribution towards the solution of earth-sculpture. Geikie argues in favour of the conclusion that there must have been six post-Tertiary glacial periods with intervening times of mildness, but as to this, and as to the extent to which glacial periods may be recognised in earlier ages, there remains much difference of opinion.

The "drift" which spreads over Northern Europe, with its boulder-clays, erratic blocks, moraines, and the like, admits of only one interpretation,—that it is the residue of glacial action. The polished and striated or often much broken rocky floor on which the deposits rest; the rounded and abraded *roches moutonnées;* the arctic marine shells found in the drift of Britain, etc., up to heights of

over a thousand feet above sea-level; the remains of boreal animals in North Temperate countries, and so on, corroborate the main conclusion.

In what are called Pleistocene times enormous continental *mers de glace* covered immense areas in Europe and North America. Great snow-fields and local glaciers accumulated especially in those areas where the precipitation of snow and rain is now most abundant, and where in some cases, as in Norway and the Alps, there are still relics of the olden times. North of Central Germany and Central Russia all Europe was buried in ice; the whole of North America north of a line between New York and the Rockies was glaciated. The mean annual temperature of Central Europe must have been lowered many degrees (perhaps 10° or 11° F. according to Penck, 5½°–7° F. according to Brückner). The climate of Southern Germany then would be like that of Northern Norway now, and so on; in short, "in glacial times a wholesale displacement of climatic zones took place." *

It is some progress, then, towards a clearer interpretation of the earth, that what were by older geologists regarded as the results of Noah's flood are now known to be the marks of a Great Ice Age—which, though very gradual in its coming and going, wrought great changes upon the face of nature and on the distribution of plants and animals.

But as the study of glacial phenomena has become more extensive and more careful, the interpretation has become more complex. Thus, the discovery of "interglacial deposits," whose fossils indicate conditions of warmth—often greater than

* Prof. James Geikie. *Trans. Victoria Inst.*, xxvi., 1892–93, p. 222.

now exist in the same localities—has forced geologists to admit the intervention of temperate stages, interrupting the monotonous tyranny of the cold. Most geologists now recognise at least two glacial epochs, and many find strong evidence of three or even more.*

Causes.—There has been no lack of theories as to the causes of the Ice Age or of the Ice Ages. Some of these theories seem too laborious and others too ingenious, but it seems doubtful if all are not premature. That is to say, we have to discover whether the post-Tertiary Ice Age, so obvious in Europe, was universal or not; and we have also to decide as to the periodicity of the recurrence of glacial conditions in older geological periods, which is almost too difficult a problem.

Since the days of Agassiz and Charpentier, the causes of the Ice Age have been sought in two directions which were to some extent hinted at by the pioneers. Some have appealed to cosmical or astronomical changes, while others have been satisfied with geographical factors.

Adhémar in 1842 seems to have suggested a theory, which was rehabilitated by James Croll (1875), that a slight alteration in the eccentricity of the earth's orbit might be the essential cause of glacial conditions.

Lyell may be taken as a representative of the view that geographical changes may have brought about glacial conditions. Depressions allowing the Arctic currents to overflow parts of the continents, elevation of large areas above the snow-line, deflections of ocean currents, and so on, have been assumed as possible causes.

* See Prof. James Geikie's *Great Ice Age* and *Prehistoric Europe.*

There are others, like Oswald Heer, who have found satisfaction in combining the cosmical and the geographical theories.

The last, or, since the stock is prolific, perhaps the *latest hypothesis* as to cause of glacial periods, is that of Professor Chamberlin who maintains that the climatic conditions which brought about ice ages arose from an impoverishment of the quantity of carbonic acid in the atmosphere.

The aim of this section has been to indicate (1) *the great change that has occurred in geology since the uniformitarians attempted to interpret glaciation apart from glaciers,* (2) *the gradual development of glacial geology, from a careful study of existing glaciers and their work to a detection of the range and routes of ancient glaciers of much greater size,* (3) *the importance of the idea of a relatively recent (post-Tertiary) Great Ice Age interrupted by intervening periods of mildness, and* (4) *the uncertainty that still obtains as to the cause or causes of this and previous glacial periods.*

THE HAND OF LIFE UPON THE EARTH.

One of the distinctive results of nineteenth-century science is the recognition of the important part which living creatures have played in fashioning the features of the earth. Each year's work has of late brought to light some fresh instance of the dominance of the hand of life, and we have devoted this section to its illustration. The central names are those of Charles Darwin and Louis Pasteur.

Plants.—From 1810 when Rennie outlined the history of Scottish peat-bogs to the latest paper on nitrifying Bacteria, the importance of plants in

relation to the earth has been more and more thoroughly appreciated.

"The sea-weeds cling around the shore and lessen the shock of the breakers. The lichens eat slowly into the stones, sending their fine threads beneath the surface as thickly sometimes 'as grass-roots in a meadow-land,' so that the skin of the rock is gradually weathered away. On the moor the mosses form huge sponges, which mitigate floods and keep the streams flowing in days of drought. Many little plants smooth away the wrinkles on the earth's face, and adorn her with jewels; others have caught and stored the sunshine, hidden its power in strange guise in the earth, and our hearths with their smouldering peat or glowing coal are warmed by the sunlight of ancient summers. The grass which began to grow in comparatively modern (i. e., Tertiary) times has made the earth a fit home for flocks and herds, and protects it like a garment; the forests affect the rainfall and temper the climate besides sheltering multitudes of living things, to some of whom every blow of the axe is a death-knell. Indeed, no plant from Bacterium to oak-tree either lives or dies to itself, or is without its influence on earth and beast and man." *

From the vegetable drift borne down often in immense quantity by rivers to the diatom ooze which accumulates in some parts of the deep-sea, there are many modern examples of additions made to the earth by plants; from the protective action of sand-binding grasses and sedges, or of mangrove belts along the coasts, to the action of many Algæ in forming deposits of carbonate of lime, there are many illustrations of processes at present going on in which plants play a part of much geological interest.

* J. Arthur Thomson. *The Study of Animal Life*, fourth edition, London, 1901, p. 25.

Almost throughout the century there has been continuous inquiry into the nature and origin of coal; much has been done in the recognition of the flowerless plants (especially club-mosses) which gave rise to it; experimental work has shown the probability of its formation under water, under great pressure, and in warm conditions; but there is still no unanimity in answering the question whether coal was formed in the site where the plants that formed it grew, or whether the material was flooded off from the old forests and deposited elsewhere.

Animals.—The influence of animal life upon the earth is also manifold. On the one hand, we see destructive agencies,—the boring sponge *Cliona* tunnelling through and through the oyster shell and tending to reduce it to sand, the Pholads and many other borers helping to break down the most solid sea-shore rocks, the crayfish and their enemies the watervoles uniting to make the river-banks collapse, the beavers cutting down trees, building dams, digging canals, and changing the aspect of even large tracts of country, and so on through a long list.

On the other hand, we see conservative agencies,—the formation of great shell-beds, the accumulation of calcareous and siliceous ooze in the great abysses of the oceans, and most strikingly the rise of coral-reefs, such as the great barrier reef of Australia which is over 1000 miles in length.

That there are great limestone beds which have been formed by the remains of marine animals is an obvious fact. They are often so thoroughly penetrated by recognisable shells of nummulites, coral, sea-lilies and molluscs, that he who runs may read their origin. In other cases, however, there are no big remains which the eye recognises at

once, and it was an important step which Ehrenberg made in 1839, when, by applying the microscope, he proved that chalk rocks were built up of the minute shells of Foraminifera. The full importance of this became plain when the *Challenger* explorers mapped out the extent of Foraminiferal ooze on the ocean floor. What is now accumulating in the abysses was seen to be the modern analogue of ancient chalk-cliffs, and the present-day representation of other than Foraminiferal limestone rocks has also been disclosed. The *Challenger Report on Deep-Sea Deposits* by Sir John Murray and the Abbé Renard (1891) may be cited as the most important outcome of this line of investigation.

The history of the study of coral-reefs, which we have been forced to omit, is a very instructive instance of gradually increasing thoroughness in the investigation of a particular problem.

The Living Earth.—Until Charles Darwin followed up Gilbert White's luminous suggestions and made a careful estimate of the work of earthworms as soil-makers, few naturalists—even—had any adequate conception of the busy world beneath their feet. Fifty-three thousand earthworms per acre, bringing ten tons of soil per annum to the surface, burying thousands of leaves and thus forming vegetable mould, bruising the particles into fineness, and by their burrows acting as ploughers before the plough,—facts like these, which Darwin substantiated with his consummate patience, made it plain that these humble creatures must be regarded as among the most useful and important animals.

But we must add details to our picture of the earthworms in their burrows; there are the moles and the sharp-toothed centipedes both persecuting

the worms, there are burial beetles excavating beneath the corpse of bird or mouse, weevils and wireworms destroying the roots of plants—and scores of other more or less subterranean animals. Then the impression of the living earth begins to grow upon us. Moreover to the business of animals we have to add that of plants,—the curving movements of rootlets, the spreading growth of underground stems, and the sprouting of seeds.

Real, however, as all this visible activity is, it is not that on account of which we have ventured to speak of the living earth. The phrase is even more thoroughly justified by work which is done by the *Bacteria* of the soil, and the recognition of this—dating from Pasteur—may be fairly called one of the characteristic achievements of the nineteenth century. It has led to a vivid realisation of the great fact of the circulation of matter.

THE PROBLEM OF PETROGRAPHY.

Microscopic Analysis.—Just as the biologist analyses the body of an animal into organs, tissues, and cells, and ends with a study of the complex organic substances therein contained, so the geologist distinguishes different kinds of rocks—limestone, basalt, granite, and so on, proceeds to describe the fine structure of each, and ends with a determination of the chemical composition of the several constituents. In a general way, petrology is to geology what histology is to anatomy,—an analysis of microscopic structure; and just as the study of histology inevitably leads to the study of histogenesis—that is, how the different tissues are developed—so petrology will only be completed when the origin as well

as the nature of rock-structure is known. In a few cases the problem is easy of solution, as when it is seen that some kinds of limestone are almost entirely composed of the shells of Foraminifera; in most cases the problem is all unsolved.

All that we can do in this section is to indicate some of the important steps which have led to the present vigorously progressive science of petrology or petrography.

Early Methods.—In 1800 Fleurian de Bellevue recommended the microscopic study of powdered fragments of rock, and Cordier, in 1815, resorted to this primitive device, and succeeded after much labour in proving that basalt was made up of several minerals. In the fourth decade of the century Ehrenberg began to apply the microscope to minute splinters and powdered fragments of various non-crystalline rocks, and showed that some of these were almost entirely composed of shells of minute animals or plants, e.g., Foraminifera and Diatoms. The step was important in itself and not less in its suggestive value.

About the middle of the century G. Bischof published his text-book of chemical and physical geology (1848–55), in which he compared the earth to "a great chemical laboratory." Although he pushed chemical interpretations to an extreme, he suggested a point of view which in later days has seemed to many like a Pisgah. From Bischof and Bunsen to the scientists of to-day there is a long list.

The Section Method.—It is said that the first to suggest and arrange the method of preparing thin sections of rocks was William Nicol, the inventor (1829) of the most useful prism of Iceland spar that bears his name. A description of his method

of making sections was published in 1831.* But these early hints had little result, and it seems fairly certain that the first to use and appreciate the method of studying thin rock-sections in transmitted line under the microscope was Dr. H. Clifton Sorby of Sheffield (1850), who had been stimulated by the sight of a collection of Nicol's preparations which had been preserved and added to by Alexander Bryson, an optician in Edinburgh.

Professor Zittel notes that, in 1852, Oschatz exhibited in Berlin a series of microscopic sections of rocks which he had made, but his results seem to have been regarded as little more than curiosities. A proof of the value of the method was needed, and that was furnished in 1858 by Sorby in a classic memoir "On the microscopic study of crystals, indicating the origin of minerals and rocks." † The next steps, and for many years almost all the important steps, were taken by continental geologists. "Even Sorby's papers, which continued to be most suggestive in this line of work, had reference only to very special points; and it may be doubted if his greatest service was not the transplanting of his ideas and methods to Germany, where they were destined to rapidly take root, and bear a fruitful harvest." ‡

It was a most fortunate thing for science that Zirkel, as a young student, made Sorby's acquaintance in Bonn in 1862, and after many walks and talks became an enthusiastic disciple, soon far to

* Henry Witham. *Observations on Fossil Vegetables*, Edinburgh, 1831. See *The Microscope*, by Carpenter and Dallinger, London (1891), p. 990.

† Quart. Journ. Geol. Soc. XIV. (1858), pp. 453-500.

‡ G. H. Williams. *Modern Petrography*, an account of the application of the microscope to the study of geology. Boston, 1886.

outstrip his master.* Undertaking, for the first time, "a systematic study of rock-sections as an end in itself," as Williams says, Zirkel began rapidly to lay the foundations of modern petrography. But with his name that of Rosenbusch must be immediately coupled; both as investigators and as teachers, they stand as the leaders of petrographical enquiry.

Among the earlier petrologists one of the most original and suggestive was Hermann Vogelsang, whose *Philosophy of Geology* (1867) is still looked upon with great admiration, who is also memorable for his persistent and successful attempts to get nearer the secret of petrogenesis by reproducing experimentally results similar to those which have occurred in nature. We cannot, however, pursue the history, and to mention even the names of those who have done great service in petrography since Zirkel and Rosenbusch became recognised leaders, would serve no useful purpose in a sketch like this. One classification has succeeded another, and no petrologist seems satisfied either with his own or his neighbour's; the question of "species" seems as puzzling as in biology; and there can be no solution until the static results of description are illumined by a theory of rock-genesis. To this, through keen struggle for existence among conflicting opinions, every year brings us nearer.

Mineralogy.—Turning to the department of petrography which restricts itself to minerals, we may note that in the early days of mineralogy the physical aspect, the study of crystalline form, specific gravity, hardness, etc., received most attention. Of especial importance was the work of Haüy who, without de-

* See F. Zirkel. *Die Einführung des Mikroskops in das mineralogischgeologische Studium*, Leipzig, 1881.

preciating the study of the chemical properties, emphasised the value of crystallography, and referred the numerous crystalline forms to a few primary types.

There was for a time a tendency among mineralogists, as among physiologists, to refuse the chemists' offer of a helping hand, but sounder views gradually prevailed. Berzelius (quoted by E. von Meyer) compares the mineralogist who refuses the aid of chemistry to a man who objects to use a light in the dark, on the ground that he would thereby see more than he requires to. The introduction of the blowpipe by Cronstedt was an event of much importance, and led on to the early chemical systems of Bergmann and others. But the modern study of mineralogical chemistry must date from the work of Berzelius, who in his Chemical System brought minerals into line with other inorganic compounds. The general tendency of subsequent systems of classification seems to have been to emphasise chemical composition, and it is interesting to notice the suggestions of Wurtz and others as to the collation of various minerals with organic compounds, e.g., poly-silicic acids with poly-ethylene alcohols.

Isomorphism.—Another great event in the history of mineralogy was Mitscherlich's discovery of isomorphism. N. Fuchs had previously observed that certain substances can replace each other in minerals; Mitscherlich showed that the same material might have two, three, or more crystalline forms. This set aside the exaggerated conclusion of Haüy that difference in crystalline form necessarily implies difference in chemical composition.

While Mitscherlich may be said to have proved irrefutably the connection between chemical com-

position and crystalline form, both he and Berzelius went too far in declaring similarity of crystalline form to be "a mechanical consequence of similarity in atomic constitution," or in other words that the atomic constitution of a substance could be inferred if that of one of its isomorphs was known. For Mitscherlich afterwards showed that dissimilarly constituted bodies might be isomorphous and similarly constituted ones heteromorphous, and that the same substance might crystallise in different forms. To this Scherer added "cases of the so-called *polymeric* isomorphism, which proved that elementary atoms might be replaced by atomic groups without change of crystalline form." *

Experimental.—We have already referred to Sir James Hall as the founder of experimental geology, and may here recall that in 1801 he showed the possible transformation of chalk into marble. For this was as it were the first sentence in an exceedingly interesting chapter in the history of research—the development of experimental mineralogy. Numerous experimenters—particularly well represented in France—e.g., in modern times, Fouqué and Michel-Lévy, Friedel and Sarasin—have worked at the artificial production of minerals, and have thrown much light upon the possible ways in which minerals may have been formed in nature.

NOTE ON THE SCIENTIFIC DEVELOPMENT OF GEOGRAPHY.

One of the great intellectual advances of the nineteenth century has been the scientific development of geography. Whether we recognize one science or

* E. von Meyer. *History of Chemistry*, Trans. 1891, p. 454.

twenty is merely a question of convenience; the boundaries of the sciences whose right to the name is seldom questioned,—physics and chemistry, astronomy and geology, biology and psychology, and so on,—are flexible; two or more sciences often seem confluent; and therefore it matters little whether we regard geography as a unified and well-defined department of science, or as a combination of sciences in relation to a particular problem.

According to a definition (by Dr. H. R. Mill), on which evident care has been expended, "Geography is the exact and organised knowledge of the distribution of phenomena on the surface of the Earth, culminating in the explanation of the interaction of Man with his terrestrial environment." * Dr. Mill goes on to say, "As the meeting-place of the physical and the human sciences, it is the focus at which the rays of natural science, history, and economics converge to illuminate the Earth in its relation to man. . . . The unity of geography results from viewing nature in the limited but still general aspect of the phenomena which affect the surface of the Earth."

The geographer is concerned with the atmosphere, the hydrosphere (the water-envelope), and the lithosphere (the rocky crust whether of the continents or the ocean-floors). "His first business is to define the *form,* or relief, of the surface of the *solid* sphere, and the movements, or circulation, within the two *fluid* spheres. The land-relief conditions the circulation, and this in turn gradually changes the land-relief. The circulation modifies climates, and these, together with the relief, constitute the environments of plants, animals, and men. Short of complexities,

* *The International Geography.* London, 1899. p. 2.

this is the main line of the geographical argument. In the language of Richthofen the earth's surface and man are the terminal links." *

It might seem as if geography had become a compendium of the sciences and took all nature for its province, but that is a misinterpretation of the modern extension. The fact is that geography is a synthesis of the results of many sciences in relation to a special problem; or it may be compared to a central circle intersecting a cluster of other circles which represent physics, chemistry, astronomy, geology, biology, anthropology, and so on.

Alexander von Humboldt is ranked as one of the founders of scientific geography, not merely because of his explorations, or his method of representing the relief of a country (e.g., Mexico) by cross section, or his invention of isotherms, but because he had the distinctively scientific virtue of seeing things in their inter-relations. "Humboldt's *Essai politique sur la Nouvelle-Espagne,* published in 1809, must take high rank among the efforts of the new geography as the first complete description of a land with the aid of the modern methods. Here, for the first time, we have *an exhaustive attempt to relate causally relief, climate, vegetation, fauna, and the various human activities."* † For that is geography.

But along with Humboldt there are others who should be named,—Karl Ritter of Berlin (1779–1859), "the greatest modern professor of geography," author of the famous *Erdkunde* and founder of a great school; the cartographer Berghaus,

* H. J. Mackinder. Address Section E, *Rep. Brit. Ass.*, 1895, p. 739.

† H. J. Mackinder. *Loc. cit.* p. 741.

whose great Physical atlas is an immortal monument; Perthes, "the capitalist employer of cartographers"; and the critical Oscar Peschel. From these we pass to living workers, such as von Richthofen and Penck.

One of the great results of the nineteenth century has been the development of geography as a synthetic science.

AN ILLUSTRATION OF OCEANOGRAPHY.

The whole history of Oceanography, in its various branches, has been related in great fulness by Sir John Murray in his *Summary of the Scientific Results of the Voyage of H.M.S. Challenger;* we cannot in a section do more than illustrate the fact of its rapid development in the second half, and especially in the last quarter of the nineteenth century. The illustration we take is the familiar but striking one that within a few years we have gained a wealth of information in regard to the Deep Sea, which was about the middle of the century an almost unexplored area. In spite of isolated hints which might have been followed up earlier, it was generally believed until 1860 or so, that the great depths of the ocean were uninhabitable, and there was almost no knowledge of the deposits covering the floor. A notable step was taken when Surgeon-Major G. G. Wallich, naturalist with Sir Leopold M'Clintock's North Atlantic Expedition of 1860, showed that animals lived in the abysses even below 1,000 fathoms. It is interesting also to notice that one of the impulses which gave Deep-Sea exploration a start was the purely practical desire to establish telegraphic communication between the Old and New Worlds. In binding these together, another new world was discovered.

The recognition of oceanography as a distinct branch of science may be said to date from the commencement of the *Challenger* investigations, and although the study is still in a sense in its youth, "so much has already been acquired that the historian will, in all probability, point to the oceanographical discoveries during the past forty years as the most important addition to the natural knowledge of our planet since the great geographical voyages associated with the names of Columbus, Da Gama, and Magellan, at the end of the fifteenth and the beginning of the sixteenth centuries." *

Our picture of the Deep Sea is necessarily darkly-shaded and in many respects dim and vague, but it is not wanting in precise detail. Some indication of this may be given. At great depths there is necessarily enormous pressure (at 2,500 fathoms about 2½ tons upon the square inch); it is quite calm, untouched by the severest storms; the temperature is low and uniform, often just a little above the freezing-point all the year round; the water is relatively rich in oxygen; there is practically no light, apart from phosphorescence; there are therefore no green plants and there is no secure evidence even of Bacteria; there is no depth limit to the distribution of animal life and the population includes representatives of most of the great types of animals from Protozoa up to fishes; the animals necessarily feed to a large extent upon one another, but fundamentally upon the organic débris which sinks from above, and not least upon the ceaseless rain of pelagic Protozoa which sink down from the surface as they die. A strange, silent, cold, dark, plantless world!

* Sir John Murray. Address Section E, *Rep. Brit. Ass.*, 1899, p. 790.

While the shallow-water areas down to the 100-fathom line are now known with much exactness in many parts of the globe, there is naturally much less certainty in regard to the deeper parts, though, as Sir John Murray remarks, some 10,000 deep soundings were taken in the last decade of the nineteenth century. He estimates that considerably more than half of the sea-floor (103,000,000 square geographical miles in all) lies at a depth exceeding 2,000 fathoms, or over two geographical miles. There is a relatively rapid descent along the continental slopes between 100 and 1,000 fathoms, and there are over forty known depressions of more than 3,000 fathoms. The greatest known depth is in the S. Pacific, to the east of the Kermadecs and Friendly Islands, 530 feet more than five geographical miles, or 2,000 feet more below the level of the sea than the top of Mount Everest is above it.

Direct observations with deep-sea thermometers, and indirect inferences from the electric resistance of the telegraph cables lying on the floor of the oceans, show that about 92 per cent. of the entire sea-floor has a temperature less than 40° Fahr. The surface-water cooled at the poles, spreads over the floor towards the equator, carrying with it the oxygen which makes abyssal life possible. Since the light as well as the warmth of the sun does not penetrate below the superficial layers of water, the deep-sea area is dark as well as cold. Therefore there are no plants (apart from some doubtful forms and the resting stages of two or three Algæ), and this implies that the abundant deep-sea animals depend in the long run on supplies which sink downwards from the populous surface or the crowded shore-areas.

Especially by Sir John Murray and the Abbé

Renard a most careful study has been made of the marine deposits on the ocean-floor. These are conveniently divided into two sets—(1) the terrigenous deposits, for the most part consisting of the disintegrated particles of the adjacent emerged land, and of great interest as illustrating accumulations analogous to those which formed many of the stratified rocks; and (2) pelagic deposits, which begin at an average of about 200 miles from the continental coast-lines, and are mainly composed of the shells of pelagic organisms (Molluscs, Foraminifera, Radiolaria, Diatoms, etc.), besides inorganic particles of volcanic or cosmic origin. The "Red Clay," which covers nearly half of the sea-floor, and all the deeper parts, is probably due to the chemical alteration of organic and inorganic remains during a prolonged period of slow accumulation. Sir John Murray argues that the number of sharks' teeth, of earbones and other bones of whales, and of cosmic spherules in a deposit may be taken as a measure of the rate of deposition. These bodies are most abundant in the Red Clay, probably because few other substances reach the great depths to cover them up. "One haul of a small trawl in the Central Pacific brought to the surface on one occasion, from a depth of about 2½ miles, many hundreds of manganese nodules, along with 1500 sharks' teeth, over 50 fragments of earbones and other bones of whales."

It may seem to the careless both dull and unprofitable to map out with care the sediments which are now forming on the floor of the ocean, but the importance of these maps to the geologist is immense. For it is from them that we can argue back to the history of the sedimentary part of the earth's crust,

and show how in the Triassic, for instance, there was sea where there is now the great mountain belt of the Euro-Asiatic continent, or how the great chalk deposits of the Cretaceous are the analogues of the deep sea Globigerina ooze of to-day.

SUMMARY.—*One of the great discoveries of the nineteenth century was that of the Deep Sea—almost a new world. The vast depths, the low temperature, the abundant animal population and the deposits which accumulate on the floor have been the subject of careful and fruitful study, but the vastness of the area makes it certain that much that is new still awaits the explorer of the abysses.*

BOOK THREE.

SCIENCE OF ORGANISMS—LIFE-LORE.

CHAPTER VIII.

The Deepening of Physiology.

HISTORICAL OUTLINE.

Aspects of the Organism.—The living body as a subject of scientific enquiry may be approached from many different sides. A dim personality it often seems, intelligent or instinctive in its actions; or it may live its life on a lower plane where neither of these terms is applicable. It is a unit in a family or flock, in a fauna or flora, an item in the midst of an environment, and must be studied in its inter-relations of dependence or antagonism, of co-operation or competition, of successful adaptation or failure to survive. It is a member of a race, starting in life with a multiple inheritance from many ancestors; its individual becoming must be studied in the light of its past history, its development in the light of its evolution. It is an engine, transforming matter and energy, and must be studied as a problem in dynamics. It is a chemical laboratory, in which reductions, oxidations, disruptions, constructions, explosions, and fermentations go on in manifold complexity.

In these various ways the living body may be studied, and no one of them has been disregarded by the physiologists. It is plain, however, that along some of these lines at least no secure progress could be made until the sciences on which physiological investigations depend had begun to gain clearness and stability. There could be no chemical physiology when combustion was not understood, and little physical physiology when heat was regarded as an element or as an entity. It follows that almost all analytic physiology involving chemistry and physics must be comparatively modern, and that we are not likely to find much value in the physiology of the eighteenth century or earlier except in so far as that was concerned with descriptions of the habits of the intact creature, with observations on the gross functions of organs, or with merely mechanical analysis.

Sketch of Physiological Progress.—In what are called the Middle Ages (to which, as regards biology and psychology, many people still belong) the analysis of the organism was only incipient. Comparative anatomy and comparative physiology were still embryonic. Chemistry and Physics were not yet sufficiently stable themselves to be able to help another science to a firm foothold. Yet then, as ever, men looked out upon nature with inquisitive eyes, accumulated a wealth of sense-impressions, and recorded their perceptions in more or less orderly form. Many interesting phenomena of plant and animal life were observed, and sometimes accurately described. But when the mediæval observers went beyond this, and took the more characteristically scientific step of devising general formulæ for the sequences and likenesses which they perceived, they were almost forced to do so in metaphysical terms. Their

shorthand was frankly anthropomorphic or spiritualistic; they invoked "animal spirits" and "vital spirits," "principles of life" and "*vires formativæ,*" "humours" and "temperaments." It is difficult to see how it could have been otherwise.

But as inquisitiveness became gradually more penetrating, as the organs of the body were disclosed in many other creatures besides man, as the uses of many of them were in part discovered, the spiritualistic formulæ began to appear somewhat gratuitous. Thus it is interesting to note that Mariotte (d. 1684), who compared the entrance of water into the roots of plants to its rise in capillary tubes—a shrewd suggestion—was one of the first to discard the hypothesis of "*a vegetable soul*"—as a factor in the plant's every-day functions. Harvey's demonstration of some of the factors in the circulation of the blood may be taken as one of the first of the long series of attempts to express vital phenomena in terms of mechanism—attempts which put an end to the reign of spirits, though not to the intrusion of metaphysics. The great work of Haller (1708–1777)—*Elementa Physiologiæ Corporis Humani*—represents the position of the study of the functions of the organs of the body at the beginning of the nineteenth century, and it is marked by its endeavour to reject all that could not be verified by observation and experiment.

When we pass from the work of Haller to that of Johannes Müller (1801–1858) we feel at once in a new century. Chemistry and physics had made great strides, and he calls them to his aid in his physiological work. Man was no longer studied alone, for Müller's physiology was essentially comparative. The facts of mental life were no longer kept wholly

apart from corporeal affairs, for, as Verworn notes, Müller defended even in his examination for the doctorate the thesis: *Psychologus nemo nisi physiologus.* But it is interesting to find that this genius who did so much to give physiology its modern aspect was like most of his contemporaries, a vitalist. He maintained that the functions of the body exhibited sequences comparable to those observed by the chemist and physicist in not-living bodies, yet he believed that there was in the organism a "vital force" which had to be taken account of in physiology.

Meanwhile pursuing the general trend of biological research, we may note that just as the study of the intact organism as a bundle of habits and temperaments more or less kept in order by a "*spiritus rector*" gave place to a study of the activities of particular organs—the brain, the heart, the lungs, the liver, and so on, so the resulting conception of the living creature as an engine of many parts had to be supplemented by the study of the properties of tissues (muscular, nervous, glandular, and so on), —a step which we particularly associate with the publication of Bichat's *Anatomie Générale* in 1801. Gradually, however, as the microscope was improved, the existence and importance of the little areas of living matter which we (unfortunately) call cells was recognised, and in 1838–39 Schwann and Schleiden formulated their "Cell-Theory" or Cell-Doctrine,—(*a*) that all plants and animals have a cellular structure, (*b*) that the life of all multicellular organisms (reproduced in the ordinary way) begins in a single cell—the fertilised ovum—which proceeds to build up the body by a process of cell-division, and (*c*) that the life of the whole is expressible in terms of the activities of its component

cells. One step further in analysis brings us to the characteristically modern study of the chemical and physical changes which go on in the contents of the cells, that is to say in "the physical basis of life," as Huxley phrased it, or protoplasm.

PHYSIOLOGY OF THE LIVING ORGANISM AS A WHOLE.

The Life of Living Creatures.—In the childhood—a prolonged period—of Life-Lore, attention was in great part directed to the study of the activity of the living creature as an intact whole. It is or should be so in the childhood of the individual. Life as it is lived in nature, the behaviour of the animal, its relations to other living things, the "habit" of the plant, its friends and foes,—these form part of the oldest physiology and they should still command our attention to-day.

The term physiology is too much restricted to a study of the internal economy of the organism. Just as anatomical analysis may be compared to picking a watch to pieces—an operation which dimly suggests the delights of dissection—so physiological analysis may be compared to a study of the kinetic aspect of the watch, and even when physiology becomes comparative it is still like comparing one kind of watch with another. To save the results from inexcusable partiality and incompleteness it is necessary to sound the natural history note, the recognition of organisms in the plural, as members of a pair, a family, a flock, an association, a fauna, as threads in a web of life, as agents in a complex environment. In short, it must be recognized that physiological analysis has seriously to deal with the intact living creature in its natural surroundings, with its

domestic and social relations, with its habits and adaptations, with its struggle for existence and endeavour after well-being. Physiological analysis thus completes and corrects itself in "Natural History."

Two Lessons from the Old Natural History.— The two chief lessons now to be learned from the old books on natural history are lessons of warning. (1) On the one hand we are warned against the extremely analytic method of modern biology, against the necrology which is always destroying in the effort to understand. Since our methods *force us* to abstract certain aspects of the organism, there is an undoubted risk lest we forget the unity of the organism which we take so carefully to bits; there is an undoubted risk lest we forget that what we measure and weigh and analyse belonged to a creature which had something analogous to our personality. We cannot dispense with our analysis, however, and the corrective for its partiality is simply more study of the real life of the creature in its natural environment, in other words more "Natural History," what some indeed have called "the higher physiology."

(2) On the other hand, the comparative failure of much of the old natural history—so often vague, inaccurate, and fallacious—warns us of the futility of trying to dispense with the analytic methods and their results. In proportion as our analysis is thorough so will our realisation of the life around us be vivid. To say that no one really knows a bird who has not watched it build its nest may be true; but it may be justly retorted that no one really knows a bird who does not understand the peculiarities of its respiration.

Historical Note.—The "higher physiology" or

"Œcology" (as Haeckel calls it) of living creatures is the oldest department of the science. It had its basis in the lore of the hunter and fisher, the shepherd and farmer, or further back still in that of Mowgli in the jungle.

But the old lore was much mixed with superstition, it was often inexact, and on the whole uncritical. Exact natural history is essentially modern, and, apart from a few pioneers, may be said to date from the enthusiastic observations of men like Swammerdam (1637–1680), Leeuwenhoek (1632–1723), Réaumur (1683–1757), Roesel von Rosenhof (1705–1759), Trembley (1700–1784), Schaeffer (1718–1790), Gilbert White (1720–1793), and Buffon (1707–1788).

We have placed Buffon's name last because he represents a transition between the old naturalists and the new, for while he may not have had the exactness of some of his predecessors he had a clearer vision of the wide import of his studies. As a philosophic naturalist, he deliberately set himself to a study of the habits of animals and their adaptations to their surroundings, and unified his results in the light of the evolution-idea.

It is especially the recognition of the evolution-idea that makes the difference in mood between the old and new naturalists. "Before Darwin's day the student of habits, inter-relations, and adaptations had been looked upon by his sterner brethren (anatomists, classifiers, etc.) with more or less contemptuous indulgence. Since Darwin's day, however, the study of bionomics has risen to worth and dignity." *

The study of the life of plants and animals as it

* See the author's *Science of Life*, 1899, Chapter XIV.

is lived in nature is an essential part of a general system of Biology. It began in practical lore, attained a high degree of excellence in the seventeenth and eighteenth centuries, but acquired in the nineteenth century greater dignity and definiteness especially through the influence of evolution-doctrine.

STUDY OF THE FUNCTIONS OF ORGANS.

Sir John Burdon-Sanderson dates modern physiology from the work of Johannes Müller (1801–1858). "Just as there was no true philosophy of living nature until Darwin, we may with almost equal truth say that physiology did not exist as a science before Johannes Müller. For although the sum of his numerous achievements in comparative anatomy and physiology, notwithstanding their extraordinary number and importance, could not be compared for merit and fruitfulness with the one discovery which furnished the key to so many riddles, he, no less than Darwin, by his influence on his successors was the beginner of a new era." *

Steps of Progress since Johannes Müller.—What then has been the nature of the steps of progress in regard to the physiology of organs during this period which dates from Müller? As it seems to us, the steps may be grouped under four heads:—(1) the partial elucidation of the function of organs previously enigmatical, (2) the recognition that the functions of organs, whose uses were partially known, are much more complex than was previously supposed, (3) a fuller understanding of the correlation and co-operation of the various organs in the life of the whole, and (4) the progress made in comparing analogous organs in different kinds of organisms.

* Pres. Address. *Rep. Brit. Ass.* for 1893, p. 9.

Of each of these steps we propose to give some brief illustration.

(1) *Elucidation of Enigmatical Organs.*—In the body of a higher animal there are numerous organs which take materials from the blood and get rid of these, usually in modified form, as a secretion which exudes through a duct or ducts on some internal or external surface. We call these "glands"; the liver, the pancreas, the sweat-glands, the milk-glands are familiar examples.

But there are other organs, somewhat analogous in structure, which though they take materials from the blood, and form a secretion, have no ducts. If these "ductless glands" get rid of their secretion it must be by returning it to the blood. Some of them have directly to do with the cells of the blood; thus the spleen is in mammals a grave for worn-out red blood corpuscles, while in some lower vertebrates it seems to be one of their birthplaces. But in many other cases the ductless glands do not return any cellular material to the blood, i.e., they do not form corpuscles, and what fluid material they return to the blood can only be discovered indirectly. A good example of this is furnished by the thyroid gland.

The thyroid gland is a small reddish organ, richly supplied by blood-vessels, weighing from one to two ounces in man, situated in the front of the throat on each side of the windpipe. What its precise function is we do not yet know, but very suggestive hints have been gradually accumulating of recent years, and we are certain that in spite of its minuteness it is extremely important. When it atrophies or is excised, the disease myxœdema ensues, in which the connective tissue becomes overloaded with mucinous

substance; when it is hypertrophied the resulting state is known as goitre. Associated with the enlargement there are often disturbances of the nervous and circulatory system, leading to what is known as cretinism, a state of semi-idiocy. "It is found that even if a minute part of the thyroid gland be left whilst the greater part is removed, the symptoms (which follow complete excision) do not supervene. Indeed, certain contradictory results which have been got by some observers after removal of the thyroid are explained by the fact that in some individuals there are minute detached particles of thyroid gland lying apart from the main organ; and that after the latter has been removed these detached particles may sufficiently carry on the function of the organ in relation to the blood and the nervous system to prevent the supervention of the deleterious symptoms which usually occur after its removal. Here is, then, a notable instance of the enormous influence exerted by a 'next to nothing' upon the general organism." *

The story does not, however, end here, though there is the usual need for caution in speaking of what is still, so to speak, in the melting pot. It has been shown in many cases that patients whose thyroid has been excised, atrophied, or functionally disordered, can be greatly benefited, or temporarily cured, by utilising the thyroid glands of sheep, etc., either along with the food, or by sub-cutaneous injection of the extract. This goes to show that the thyroid in its normal state forms a potent internal secretion, even small quantities of which are sufficient to keep the blood and the nervous system up to a certain standard of efficiency.

* Prof. E. A. Schäfer. Address Section I, *Report Brit. Ass.* for 1894, p. 801.

(2) *Recognition of Greater Complexity of Function.*—In the early years of the nineteenth century physicians were wont to say that the liver was an organ whose function consisted in secreting bile. In other words, a very obvious function of a big organ had been seized upon, and the demonstrable certainty of it served rather to hinder than to promote further research. That the liver does secrete bile is plain enough, but the detection of this function did not even hint at the real importance of the organ in question.

The transition towards a recognition of the more complex and manifold functions of this—the largest—gland in the body may be associated with the work of Claude Bernard (1813–1878) who demonstrated its "glycogenic function." He showed (1857) that after a meal the liver acts upon the food-laden blood, and forms glycogen or animal starch, C_{12}, H_{20}, O_{10}, H_2O, and thereafter allows this store to pass away gradually, probably in the form of a soluble sugar, in the blood, to serve as a food for the tissues, the muscles in particular. The carbohydrates digested in the food-canal enter the blood as sugars, assuming the form of dextrose, and while the amount of this in the general blood is about 0.1 per cent., it reaches 0.2–0.3 per cent. in the (hepatic-portal) veins leading from the gut to the liver after a meal rich in starch. After abundant carbohydrate food the glycogen-store in the liver may become enormous, amounting to even 12 per cent. in the fowl.

But the glycogenic function which Claude Bernard disclosed is only a second out of the many functions of the liver. Interposed as it is, a great living sponge, in the current of blood that bears soluble material from the food-canal to the heart, it has the

especial function of maintaining the approximately uniform composition of the blood, arresting superfluities and poisons, and converting harmful into harmless compounds. Any good text-book * will furnish the details.

An equally good illustration of the increasing recognition of complexity and multiplicity of function is afforded by the pancreas (the sweetbread of ruminants). This organ, which lies in the (duodenal) fold of the gut succeeding the stomach and pours its secretion into the duodenum, has been recognised—almost since digestion was understood at all—as a very important digestive organ. Its secretion acts powerfully on all the three main kinds of food,—starch, proteids, and fats,—converting starch into sugar, proteids into peptones, and fats into fatty acids and glycerine. But in spite of its importance its digestive secretion can be dispensed with, as has been proved experimentally.

On the other hand, as Minkowski and von Mering showed, a removal of the pancreas deranges the whole metabolism of the body, and the result is chronic diabetes or permanent glycosuria, marked by the abundance of sugar in the urine. As the amount of sugar can be readily measured, Minkowski was able (1889) to show with some precision the relation between cause and effect, between tampering with the pancreas and the degree of glycosuria. An additional function of the pancreas was thus discovered, or at all events rendered very probable. †

These two examples illustrate *that line of progress which has revealed an unsuspected complexity and*

* Bunge. *Physiological and Pathological Chemistry*, Trans. 1890. Lecture XVII. *Metabolism in the Liver.*

† See Bunge. *Op. cit.*, Lecture XXI., *Diabetes Mellitus.*

multiplicity of function, even in organs so familiar as the liver and the pancreas.

(3) *Fuller Recognition of Correlation.*—For ages men have been familiar with the general idea of the unity of the organism. There are many members, but there is one body; if one member suffer, the others suffer with it. At the beginning of the century (1805), Xavier Bichat recognised that "each function is linked to all the rest," and the same fact was behind the "balance of organs" of which Etienne Geoffroy St. Hilaire often spoke, and the "division of labour" on which Henri Milne-Edwards insisted.

As long as we keep to a general view, the facts seem clear enough. That certain organs should be mutually dependent follows from their nature; muscles are dependent on the nerves which stimulate them and the blood vessels which bring them food; the health of the brain or of any other part is affected by that of the liver whose fundamental function it is to be a food-filter and to keep the composition of the blood approximately constant. Facts like these are necessary consequences of the way in which the organism is made.

We get nearer a realisation of what correlation means, perhaps, when we notice the facts of functional compensation. If one lung or one kidney go out of gear the other may do double duty; if a thyroid gland be extirpated an accessory thyroid body may begin to take its place, and grow large in so doing; if a lobe of a kidney or liver has to be removed there may be a compensatory increase of function in the remainder.

But let us briefly refer to some less familiar facts which bring out more clearly the intimate correlation which makes the whole body one.

As has been noticed in the preceding section, the discovery of internal secretions, like those of the thyroid and the pancreas, shows that various organs of the body act on the blood passing through them in some specific way which is essential to the health of the whole. Even at the beginning of the century (1801) Legallois had a prevision of this; in 1857 it was brought into prominence by Claude Bernard's discovery of the glycogenic function of the liver; in 1889 it was re-emphasised when von Mering and Minkowski showed that the pancreas, besides being a digestive gland, acted as a regulator of the quantity of sugar produced or destroyed in the organism.

When the reproductive organs come to maturity, changes ensue in many parts of the body which bear witness to an intimate correlation, though we are unable to follow the physiological links. The larnyx, the hair, the milk-glands, and many other structures feel the influence. Conversely, the removal of the reproductive organs is followed by changes widespread throughout the body—penetrating even into the bones. Observations on the correlation between the reproductive organs and the antlers of stags (Rörig) are now so well-established, that one who has given attention to the matter could predict from a peculiarity of the antlers the state of the male organs, or could argue from the appearance of antlers in a female as to the abnormality of the ovaries.

To sum up, there appears to be a noteworthy step of progress in the discovery of intimate correlations previously unsuspected, and in the (incipient) investigation of the manner in which these are brought about. It implies a deeper realisation of the unity of the organism.

(4) *Progress of Comparative Physiology.*—As

far back as the second century we find Galen dissecting and experimenting on pigs and monkeys, and arguing thence to man, then a forbidden subject to biological analysis. But apart from such premonitions there was practically no comparative physiology until Johannes Müller showed that organisms of high and low degree threw light on one another. Prompted by this great master there have been many students of comparative physiology, though few have given themselves wholly to it. Thus comparative physiology lags far behind comparative anatomy; and no one has done for the former what Gegenbaur, for instance, has done for the latter. This is partly due to the intrinsic difficulties of dealing with the physiology of the lower animals (not to speak of plants) where division of labour is less marked. And another reason, as we have pointed out elsewhere,* is that the zoologist rarely knows enough chemistry, or the chemist enough zoology, to enable either to contribute much to comparative physiology.

"One zealous worker in the latter part of the Victorian era deserves to be commemorated, C. F. W. Krukenberg. He realised the dignity of the problem to which he set himself, and the results recorded in his *Studien* and *Vorträge* remain a monument to the industry of an unfortunately short life." † But the example he set is being enthusiastically followed by men like Cuénot, Verworn, and Loeb, and the contributions of older workers like Kowalewsky and Metchnikoff help to sustain the Müllerian tradition.

As an illustration of the value of comparative work we may refer to another of the enigmatical organs of the human body—the thymus gland. In

* *Science of Life*, 1899, p. 57.
† Thomson, *loc. cit.*, p. 57.

embryonic life it arises in the neck and grows down into the chest; it continues to grow after birth, but in adult life it gradually shrivels till its size is inconsiderable. It is one of the ductless glands, and is vaguely supposed to have some specific influence on the blood.

Since Kölliker discovered its endodermic origin in mammals from the epithelium of a gill-pouch, and stated that the original epithelial cells give rise to lymph cells or leucocytes, two views have been held regarding this puzzling organ. "On the one hand, Stieda and His have maintained that the leucocytes which always form integral parts of the thymus soon after its first origin have migrated thither from the exterior, possibly from the mesoblast. In this conclusion they have been supported by the researches of Dohrn, Gulland, and Maurer, and by almost every text-book of embryology and comparative anatomy published since 1879. On the other hand, Kölliker has stoutly maintained his original position, and the results of his investigations have been emphatically confirmed by Prenant, Oscar Schultze, and Beard." *

In short, it has been known for a long time that the thymus arises in the neck region of vertebrates in connection with a pair or more of gill-clefts, and that, at an early date in life, it is rich in leucocytes or white blood corpuscles, which some believed to have been born there, while others regarded them as migrants from elsewhere. It was also known that in many mammals, it degenerates after youth is over, being for instance large in the calf, but small in the cow. Generally speaking we may also say that the thymus was known to be more abundantly represented in lower than in higher vertebrates.

* J. Beard, *Lancet*, January 21, 1899.

The last impression has been made more exact by the zoological embryologists who have shown that there are 28 thymus rudiments in the lamprey, 14 in the shark, 10 in the skate, 6 in the lizard, 2 in birds and mammals. This diminished representation in the higher vertebrates suggested the idea that the thymus might be an organ specially adapted for the phagocytic protection of the gills from the invading bacteria, or from the effects of other parasites or of injuries. If this be so, we can understand why the thymus should be less represented in the higher vertebrates—Reptiles, Birds, and Mammals—in which there is no trace of gills, in which, moreover, other structures, such as the palatal and pharyngeal tonsils have, according to some authorities (Stöhr, Killian, Gulland) become garrisons of protective phagocytes, most strategically disposed.

At the beginning of 1899, however, Dr. John Beard published a short paper, announcing his discovery that leucocytes appear in the thymus rudiments of the skate (*Raia batis*) at a time when the spleen has no existence, when there are, apart from the thymus, no lymphoid structures of any sort. Cradled in the thymus, the leucocytes soon begin to emerge and migrate elsewhere.

The conclusion that the thymus is the original cradle of the white blood corpuscles of the body requires to be confirmed and extended, but it is at least a good illustration of the way in which comparative study may throw welcome light on the physiological puzzles of the human body.

Experimental.—More generally it should be noted as characteristic of the second half of the nineteenth century that physiological investigation became more and more experimental in its method. We allude

especially to the precise application of chemical and physical methods to physiological problems. On the chemical line, the researches of Wöhler, Liebig, Claude Bernard, Pettenkofer and Voit, Ludwig, Pflüger, Kühne, Hoppe-Seyler, Bunge, Halliburton, Kossel, Heidenhain, and many more have been momentous; on the physical line we have especially to remember the achievements of Weber, Volkmann, Helmholtz, du Bois-Reymond, Marey, Fechner, Ludwig, Brücke, Pflüger, Foster, and Burdon-Sanderson. But both lines of work have been prosecuted by so many that it is almost invidious to mention names at all.

PHYSIOLOGY OF TISSUES.

The Beginnings of Tissue.—The simplest living creatures are single corpuscles of living matter, structurally comparable to the individual unit-areas or cells which build up the body of a higher plant or animal, but functionally different since each one is necessarily "physiologically complete in itself," while the cell of a more complex creature shows more or less restriction of function as the result of the division of labour in the body.

Even when we pass a step upwards to the simplest multicellular organisms, such as the beautiful spherical colony or community of cells called *Volvox,* we do not yet find tissues. The members of the community, though numerous, are almost quite like one another; there is little or no division of labour.

A step higher, however, in the more complex Algæ and Fungi among plants, and in sponges among animals, we find tissues, as it were, a-making. In a sponge, for instance, we may see a number of elon-

gated, spindle-shaped, contractile cells arranged in a ring around one of the openings,—clearly representing the beginning of a sphincter muscle. Tissues are aggregates of more or less similar cells with at least one predominant function in common.

Bichat.—It was in 1801, at the threshold of our period, that Xavier Bichat published his *Anatomie Générale* which included an analysis of the body into its component tissues—muscular, nervous, glandular, connective, and so on,—and furthermore a development of the idea that the functions of organs might be expressed in simpler terms, namely, in terms of the properties of the tissues. We may take this great work as the foundation-stone of the physiology of tissues, the study of which has occupied no small part of the energy of physiologists throughout the century. The literature of research on muscular or contractile tissue alone would fill a library. Since it is necessary to restrict ourselves to one illustration, we have chosen that which is perhaps most generally interesting,—the physiology of nervous tissue.

Nervous Tissue.—Aristotle does not seem to have had any idea of the physical basis of his own genius; he did not know the function of the brain, nor was he clear as to difference between nerves and sinews. The contrast between this primitive ignorance—on the part of one of the greatest minds the world has known—and the knowledge of the nervous system possessed by physiologists to-day is remarkable, but even more remarkable is the relative recentness of that knowledge. Guesses and hints there may have been, but the elementary distinction between sensory and motor nerves was unknown a hundred years ago.

At the beginning of the nineteenth century it was

well known that nerves stimulated and controlled muscular activity, that the nervous system was the seat of feeling and thought, that different parts of the brain had different functions, and so on, but the mechanism of nerve ganglia and nerve fibres was almost unknown, though some physiologists were pondering over it. Indeed the history of the subject may be said to begin with 1811, when an English surgeon, Charles Bell, privately published a pamphlet setting forth a "New Idea," that "the nerves are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office as they are in origin from the brain." As Sir Michael Foster has said, "our present knowledge of the nervous system is to a large extent only an exemplification and expansion of Charles Bell's 'New Idea,' and has its origin in that." *

"During the latter part of the present century, and especially during its last quarter, the analysis of the mysterious processes in the nervous system, which issue as feeling, thought, and power to move, has been pushed forward with a success conspicuous in its practical, and full of promise in its theoretical, gains. That analysis may be briefly described as a following up of threads. We now know that what takes place along a tiny thread which we call a nerve-fibre differs from that which takes place along its fellow-threads, that differing nervous impulses travel along different nerve-fibres, and that nervous and physical events are the outcome of the clashing of nervous impulses as they sweep along the closely-woven web of living threads of which the brain is made. We have learnt by experiment and by observation that the pattern of the web determines the play of the impulses, and we can already explain many

* Pres. Address. *Rep. Brit. Ass.* for 1899, p. 11.

of the obscure problems not only of nervous disease, but of nervous life, by an analysis which is a tracking out the devious and linked paths of nervous threads. The very beginning of this analysis was unknown in 1799."

We have noticed that in 1811, Charles Bell (1774–1842) announced his "new idea" that the posterior or dorsal roots of the spinal nerves are sensory in function (conducting impulses centripetally), while the anterior or ventral roots are motor in function (conducting impulses centrifugally),—a conclusion afterwards proved experimentally by Johannes Müller.

The next great step was due to Johannes Müller (1801–1858), and was expressed in his doctrine of the specific energies of the nerves and sense-organs (1826). Different kinds of stimuli applied to the same sense-organ always evoke the same kind of sensation; or, conversely, one and the same stimulus or the same external phenomenon, evokes different sensations by acting on different organs. As Bunge says: * "The phenomena of the outer world, therefore, have nothing in common with the sensations and ideas they call forth in us, and the states and processes of our own consciousness are alone immediately subject to our observation and recognition."

Müller was right in his conclusion that, however a particular nerve is stimulated, the message is always of the same kind as that which is normally delivered by the nerve; an unusual stimulus to the optic nerve will result in visual sensation. But he was wrong in ascribing the specific effects to the

* *Physiological and Pathological Chemistry.* Trans. 1890, p. 12.

nerves instead of to the nerve-centres with which they are associated.

It was recognised by Vulpian (1866) that "all nerves—sensory, motor, vaso-motor, and others—have the same properties, and are only distinct in their effects. This question is of the highest importance for general physiology. It dominates the whole physiology of nerve-fibres." * "Many observations made since Vulpian wrote have shown that a nerve has no functions more *specific* than those of a telegraph wire. It conducts impulses and is incapable of tampering with the messages which it transmits." †

Since the days of Müller the progress of this department of physiology has depended on work along several distinct lines. There is, on the one hand, the more experimental method which aims mainly at localising certain functions in certain parts of the system; from Willis and Flourens (1794–1864) among the early workers, to Ferrier, Fritsch, Hitzig, Munk, Goltz, and Horsley, there has been a remarkable record of achievement. This has depended partly on experimentation with living creatures, and partly on the observation of pathological conditions, i.e., on the correlation of abnormal functions studied during life with the abnormal structure revealed on post-mortem examination.

There is, on the other hand, the histological path—"the attempt by microscopic analysis to find a way through the extraordinary maze of cells and fibres which form the brain and spinal cord. Albert von

* Quoted by Dr. Alex. Hill. *Introduction to Science*, 1900, p. 118.

† *Ibid.*, p. 118.

Kölliker was one of the most illustrious pioneers, and even as veteran he has not ceased to lead. No small part of the progress, however, has been due to the discovery of new methods, which we especially associate with the names of the Italians, Golgi and Marchi, and the Spaniard, Ramón y Cajal." * This method of investigation has been aided by embryological studies in which the development of the various parts and elements has been worked out, and by comparative anatomical studies which show the increasing complexity of nervous structure as we ascend the series.

From very early stages it is evident that the central nervous system consists of two classes of elements—(1) very numerous cells (spongioblasts) which serve for the support (neuroglia) of the essential nervous tissue, and (2) less numerous mother-cells of nerve-cells or neuroblasts.

Each neuroblast gives origin (in higher animals) to an "axis-cylinder process" or nerve-fibre, and a number of dendritic "protoplasmic processes." The nerve-fibre passes from the cell in the central system to its distribution, which may be in the nerve-cord itself, or on muscle, or in peripheral sense-organs.

Within what is called "the grey matter" of the brain and spinal cord, these nerve-cells lie in a network or feltwork of extraordinary complexity formed by the branching of the processes of the cells and fibres. Whether the fine twigs of the branches of adjacent cells end freely, or are in contact or continuity with one another, or are in some cases independent and in other cases inter-united, remains a subject of discussion. But the majority of histologists have accepted the "Neuron-Theory" which

* Thomson, *Science of Life*, 1899, p. 62.

Waldeyer stated in 1891:—" A nerve fibre is an essential part of the cell with which it is continuous and the cell, its processes, the nerve fibre and the collaterals which arise from the nerve fibre collectively form a neuron or structural nerve-unit." *

The kernel of the neuron-theory is in the conclusion that nerve-cell and nerve-fibre represent a single cell, that the axis-cylinder of the nerve-fibre, with its collateral branches and terminal ramifications, is, like a dendritic process, an outgrowth from the nerve-cell. Verworn speaks of the triple foundation of this doctrine,—(1) anatomical, (2) embryological, and (3) experimental.

(1) Remak in 1838 and Helmholtz in 1842 had shown the continuity of nerve-cell and nerve-fibre; Deiters distinguished the axis-cylinder process from the protoplasmic processes; the methods of Golgi and Ramón y Cajal, of Ehrlich and Nissl, helped the histologist to find his way in the maze; the work of Kölliker, Waldeyer, Retzius, Lenhossék, Van Gehuchten, Biedermann and many more gradually led the majority to the idea of the unity of the neuron.

(2) To Prof. Wilhelm His in particular we owe our knowledge of the development of a mother-nerve-cell into a neuroblast and of this into a nerve-cell, with a nerve-fibre, and dendrites. There is an unforgetable figure by Ramón y Cajal, which shows on the upper line the increasing complexity of a certain kind of nerve-cells in the series—frog, lizard, rat, man; while the lower line shows five stages in the individual development of a neuroblast; the result showing the general parallelism between individual growth and racial progress.

* Sir William Turner, Pres. Address, *Rep. Brit. Ass.* for 1900.

(3) A third foundation for the neuron-theory has been afforded, as Verworn points out, by experimental work. As early as 1852 Waller showed that a nerve-fibre degenerates when its connection with the associated nerve-cell is severed; Von Gudden, Von Monakow, Ranvier, Forel, and many others have continued the enquiry, and have demonstrated that the cell as well as the fibre suffers when their connection is broken. This points again to the unity of the neuron.

The last decennium of the nineteenth century has been rich in investigations prompted by the neuron theory. (*a*) The internal complexity of the nerve-cell and its processes has been disclosed by many different methods; it is enough to say that the nerve-cell is a microcosm in itself. (*b*) The difficult question of the inter-relations of adjacent neurons has been much discussed, and although it is certain that the neurons of adult animals have intimate functional inter-relations, it is difficult to make any general statement in regard to the exact nature of the contact or continuity. (*c*) It is necessary to have some hypothesis in order to interpret the making and breaking of the conducting paths through the jungle-like complexity of the grey matter and many suggestions have been made, discarded, rehabilitated, and again rejected. In no other way, until an epoch-making discovery is made, can there be progress. Thus, Prof. Mathias Duval in his "histological theory of sleep" suggested that the dendrites of the cerebral cortex contract, like the pseudopodia of an Amœba, when the cell is fatigued, that sleep (with its dislocated consciousness) ensues, and that during the period of rest the dendritic processes stretch out again into contact with their neighbours. The idea that the cells

of the cortex are "like a group of Amœbæ having a talk together," as it has been romantically expressed, may be a fascinating one, but there is very little scientific evidence in its favour.

(*d*) Not less difficult to answer is the question "What part do the nerve-cells play in relation to the conducting or impulse-transmitting function of the nerve-fibres?" One extreme is expressed in the answer—for which the explorer Nansen was first responsible—that the substance of the nerve-cell or ganglion-cell has merely a nutritional value, but this is almost contradicted by the facts known in regard to nerve-fatigue. The other extreme is expressed in the answer, for which there is much more to be said, that the specific-nervous functions have their seat in the substance of the ganglion-cell. Between these may be placed the view that the nervous processes have their physical basis in a functionally homogeneous fibrillar substance continuous through the whole of the neuron. This again is one of the problems handed on unsolved to the twentieth century.

(*e*) But we must not pass over the line of investigation which first became prominent in a research by Prof. Hodge—"A microscopical study of changes due to functional activity in nerve cells" * and has since been pursued by many,—Mann, Lugaro, Nissl, Goldschneider and Flatau, Marinesco, Fick, Guerrini, and others. Not many years ago the possibility of demonstrating the structural effects of nerve-fatigue would have seemed an impossibility; it may now be called an achievement. Whether we follow Hodge in showing the difference between the

* *Journal of Morphology*, VII., 1892.

fresh bee's brain in the morning and the fatigued bee's brain in the evening, or the results of others who have investigated the fatigue-conditions in various nerve-centres, we find an impressive set of facts, showing how fatigued nerve-cells pass into a state of collapse from which recovery may be rapid, long-delayed, or impossible. That the enquiry has its bearings on mis-education, over-pressure, strain, and worry, and the like is obvious enough. But as to the particular components of the neuron on which the fatigue-state most essentially depends we are still in doubt.

We have been particularly indebted in this section to a lecture by Prof. Max Verworn * who supports the neuron-theory enthusiastically, and we should also refer to another by Hoche,† who maintains that the functional unity of the neuron must be recognised, though its histological unity is in adult animals undemonstrable.

"The kernel of the neuron-theory is that the body of the ganglion-cell with its nerve-fibre and its dendrites is a cellular unity. . . . The anatomical and physiological investigations of the last decennium have not been able to shake this. . . . Whether the individual neurons are merely connected by contact, or in many cases are continuous by the anastomoses of fibrils or protoplasmic concrescence, is a minor question, affecting the neuron-theory not more than the fact of intercellular bridges affects the cell-theory. . . . The conception of the neuron stands, unless it can be shown that what is regarded as a cellular unity is really composed of several cells. . . . The neuron is varied in its

* Max Verworn, *Das Neuron in Anatomie und Physiologie:* Jena, 1900, p. 54.

† A. Hoche, *Die Neuronen-lehre und ihre Gegner:* Berlin, 1899.

form and function, but it remains an uncontroverted fact." *

Tens of thousands of neurons go to form the brain and spinal cord of higher animals, and it is certain that they are not homogeneous in structure or uniform in function throughout. To some degree, at least, there is a localisation of psychical functions.

"The foundation of a scientific basis for localisation dates from 1870, when Fritsch and Hitzig announced that definite movements followed the application of electrical stimulation to definite areas of the cortex in dogs. The indication thus given was at once seized upon by David Ferrier, who explored not only the hemispheres of dogs, but those of monkeys and other vertebrates." † Motor and sensory areas were distinguished, and the researches of Munk, Beevor, Horsley, Goltz, Schäfer, Flechsig, and many others have contributed to the preliminary mapping out of the brain.

Apart from centres of special sense and motor centres, Prof. Flechsig has distinguished (1896) "association-centres," which he speculatively regards as engaged in the higher intellectual operations. While this interpretation remains quite uncertain, we owe much to the observations by which Flechsig has shown that different centres in the human brain attain their perfect structural development at different periods. "When a child is born, very few of the fibres of the cerebrum are myelinated (let us say, structurally completed), and we have thus an anatomical explanation of the reason why an infant has so inactive a brain and is so helpless a creature. It

* Freely translated from Verworn, *op. cit.*

† Sir William Turner, Address Section H, *Rep. Brit. Ass.*, 1897, p. 785.

will be of special interest to determine whether in those animals which are active as soon as they are born, and which can at once assume the characteristic attitude of the species, the fibres of the cerebrum are completely developed at the time of birth." *

THE LIFE OF CELLS.

The Cell-Doctrine.—A recognition of the importance of cells as structural and functional units was one of the distinctive biological steps of the nineteenth century.

"Without hesitation I should say that one of the greatest achievements of biology in the nineteenth century was the recognition that plants and animals are composed of cells, or, more generally expressed, of numberless very minute, elementary organisms. By the co-operation of famous biologists—I mention only Purkinje, Schleiden and Schwann, Hugo von Mohl, Nägeli, Remak, Kölliker and Virchow, Brücke, Cohn and Max Schultze—our knowledge of the organisation of living substance has been greatly extended and deepened. In the theory of cells and protoplasm, anatomy and physiology secured a firm foundation similar to the theory of atoms and molecules in chemistry." †

Speaking of the cell-theory, Prof. E. B. Wilson gives a similar verdict, "No other biological generalisation, save only the theory of organic evolution, has brought so many apparently diverse phenomena under a common point of view, or has accomplished more for the unification of knowledge." ‡

The cell-doctrine includes three propositions:—(1) *Morphological,* that all living creatures have a

* Sir William Turner, *loc. cit.*, p. 785.

† Prof. O. Hertwig, *Die Entwicklung der Biologie im 19 Jahrhundert :* Jena, 1900, p. 5.

‡ *The Cell in Development and in Inheritance,* 2nd ed., 1900.

cellular structure, i.e., are either single corpuscles of living matter (the unicellular Protozoa and Protophytes), or are built up of a large number of such corpuscles and modifications of these; (2) *Embryological,* that every organism, reproduced in the ordinary sexual way, starts in life as a fertilised ovum, which divides and re-divides into a coherent embryonic mass of cells,—the beginning of a body; and (3) *Physiological,* that the functions of a multicellular organism are to some extent expressible in terms of the activities of its component cells. *

The history of microscopic analysis will be alluded to in the next chapter, but it may be noted here that the cell-doctrine is a fine example of a generalisation reached gradually by work done along many different lines and by many investigators. We may particularly associate its formulation with the work of Schleiden (1838) and Schwann (1839), Goodsir (1845) and Virchow (1858), but there were many others who contributed to the result. As to the different paths pursued, we should notice (*a*) the analysis of the body into tissues (Bichat), (*b*) the discovery and study of unicellular organisms (e.g., the investigation of Bacteria and Infusorians by Leeuwenhoek, of the Amœba by Roesel von Rosenhof, of Foraminifera by Dujardin), (*c*) the recognition of the unicellular nature of ovum and spermatozoon and of the cleavage that follows fertilisation, and (*d*) the gradual disclosure of the cellular structure of organisms,—first in plants, and then in animals.

Cellular Physiology.—This is a distinctively modern study and is still embryonic. Its central idea is that of expressing vital processes in terms of

* See *The Science of Life*, p. 103.

the activities of the cells. "Consideration of the individual functions of the body urges us constantly toward the cell. The problem of the motion of the heart and of muscle-contraction resides in the muscle-cell; that of secretion in the gland-cell; that of food-reception and resorption in the epithelium-cell and the white blood-cell; that of the regulation of all bodily activities in the ganglion-cell. If physiology considers its task to be the investigation of vital phenomena, it must investigate them in the place where they have their seat, i.e., in the cell." *

The central idea of cellular physiology was clear long before its realisation began to be effected. In 1838, Schleiden said: "Each cell leads a double life: an independent one, pertaining to its own development alone; and another incidental, in so far as it has become an integral part of a plant." In 1839, Schwann said: "The whole organism subsists only by means of the reciprocal action of the single elementary parts." In 1858, Virchow said: "Every animal appears as a sum of vital units, each one of which bears with it the characteristics of life." But, although the general idea was thus more or less clear at the dates cited, the special study of the physiology of the cell is much more modern.

One of the shrewdest and keenest of the pioneers of cellular physiology was Prof. John Goodsir, who in 1842 communicated to the Royal Society of Edinburgh a memoir on secreting structures, "in which he established the principle that cells are the ultimate secreting agents; he recognised in the cells of the liver, kidney, and other organs the characteristic secretion of each gland. The secretion was, he said, situated between the nucleus and the cell wall. At

* Max Verworn, *General Physiology*, trans. 1889, p. 48.

first he thought that, as the nucleus was the reproductive organ of the cell, the secretion was formed in the interior by the agency of the cell wall; but three years later he regarded it as a product of the nucleus. The study of the process of spermatogenesis by his brother, Harry Goodsir, in which the head of the spermatozoon was found to correspond with the nucleus of the cell in which the spermatozoon arose, gave support to the view that the nucleus played an important part in the genesis of the characteristic product of the gland cell." * This is in general agreement with the modern conclusion that the nucleus is the trophic centre of the cell.

Following Verworn, one of the most enthusiastic advocates and students of cell-physiology, we may briefly indicate some of the paths of investigation that have been pursued with success.

(*a*) Unicellular organisms offer, as it were, a natural analysis of the higher creatures. Types of cell which occur in complex combinations in multicellular organisms may be studied in isolation in the unicellular forms. The study of their normal behaviour has led to many interesting results, e.g., as regards amœboid movement and ciliary action.

(*b*) Much has been done in the way of studying the reactions of unicellular organisms to diverse artificial stimuli of heat, light, and chemical re-agents, as may be seen by a reference to the first two volumes of Prof. Davenport's *Physiological Morphology*.

(*c*) Microscopic vivisection-operations—to which the most pronounced humanitarian can offer no objections, since there can be no question of pain nor even of the destruction of life—have disclosed some

* Sir William Turner, Pres. Address, *Rep. Brit. Ass.*, 1900, p. 15.

interesting facts, e.g., that a fragment of a Protozoon, if bereft of any representative of the nucleus, will show contractility and irritability for a short time, but has no power of nutrition, growth, or recuperation. The work of Gruber, Balbiani, Hofer, and Verworn on this by-path is of especial importance; and with it we may associate the "tricks with eggs" which are played by the now numerous experimental embryologists, such as Roux and O. Hertwig, Herbst and Driesch.

(*d*) Such organisms as Flowers of Tan (*Æthalium* [*Fuligo*] *septicum*) afford large masses of relatively undifferentiated living substance which have been studied by the physiological chemist. And similarly, it is possible to obtain quantities of Protozoa, Protophytes, leucocytes, spermatozoa, ova, etc., in which structural differentiation is only implicit. "A great variety of favourable research-objects are also found for microchemical investigation, although thus far, since the methods are still little developed, only the first beginning in this direction has been made. The labours of Miescher, Kossel, Lilienfeld, Loew, and Bokorny, Zacharias, Schwartz, Löwit, and others, have already proved that the microchemical investigation of the cell has before it a rich future." *

AS REGARDS PROTOPLASM.

The earlier observers, from Dujardin and Von Mohl to Max Schultze, were well aware that the cell contained or was a minute mass of substance, often viscid, often vacuolar, often apparently homogeneous,

* Verworn, *op. cit.* p. 51.

often full of granules. But they had little idea of the intricate complexity of the cell-substance, which Virchow has lived to realise and in part to elucidate. Perhaps it is to Brücke (1861) that we should trace back the beginning of the recognition that the cell-substance is anything but homogeneous, anything but like white of egg. We have elsewhere * sketched some of the steps which led to our present realisation of the complexity of the cell-substance, which some compare to a network, others to a tangled coil of fibrils, others to a gelatinous matrix with embedded granules, and others to a foam or emulsion. It seems probable enough that one and the same cell-substance may at different times exhibit different complexities of structure. But the important fact is the one, to which more perfect lenses, more rapidly acting fixatives and subtler staining re-agents have led modern workers, that the cell has a complex structural organisation.

What is meant by Protoplasm.—The term protoplasm, which Huxley defined as "the physical basis of life," is often used topographically to include the whole of the physically complex cell-substance. It is also employed as the equivalent of cytoplasm; i.e., for the complex cell-substance *minus* the nucleus. In another usage it means the whole cell-substance in so far as that is actively concerned in vital processes, that is to say, the cell-substance *minus* obviously lifeless inclusions (metaplasm). There are some again who try to confine the term to designate the genuinely living stuff, and this would be most convenient were it not for the unhappy fact that we are at present unable to isolate that genuinely living stuff, or even to be sure that there is any one stuff that

* *The Science of Life*, 1899, Chap. IX.

could be isolated. Therefore, it seems advisable to keep to the cautious vagueness of Huxley's phrase, protoplasm is the physical basis of life.

There are three slightly different physiological conceptions of protoplasm at present in the field. (*a*) Some regard protoplasm as a substance analogous to a ferment, capable of acting on less complex material which is brought within its sphere of influence. It is the strange characteristic of a ferment, like diastase or pepsin, that it can act on other substances without being itself essentially affected by the changes it induces, and that a minute quantity can continue its work with a power which seems to have little direct relation to its amount.* (*b*) Others have suggested that protoplasm is, as it were, the central term in a complex series of chemical changes, itself the seat of continual change, ever being unmade and remade.† (*c*) Others again have suggested that there is probably no one thing that can be called protoplasm, for vital function may depend upon the interactions or inter-relations of several complex substances, none of which could by itself be called alive. Just as the secret of a firm's success may depend upon a particularly fortunate association of partners, so it may be with vitality.‡

As to the chemical composition of the physical basis of life, physiologists are not at present in a position to make many general statements.

"Just as very different structural constituents may be distinguished in living substance, so very different

* See Sir J. S. Burdon-Sanderson, Pres. Address, Section D, *Rep. Brit. Ass.* for 1889, pp. 604–614.

† See Sir Michael Foster, Article, *Physiology*. *Encycl. Brit.*

‡ See E. B. Wilson. *The Cell in Development and Inheritance*, 1896, new ed., 1900.

chemical bodies are present. The elements of which they consist are only such as exist in the inanimate world also, but their number is small, and it is chiefly the elements having the lowest atomic weights that compose living substance. A special vital element does not exist, but the compounds in which these elements occur are characteristic of living substance, and in great part are absent from the inorganic world. They are, first of all, proteids, the most complex of all organic compounds, which consist of the elements carbon, hydrogen, oxygen, nitrogen, and sulphur, and are never wanting in living substance. Further, there occur other complex organic compounds, such as carbohydrates, fats, and simpler substances, all of which either are derived from the decomposition of proteids or are necessary to their construction; and inorganic substances, such as salts and water; the latter gives to living substance its requisite liquid consistency." *

It has to be remembered that living substance must be killed before it is chemically studied, and that we have no means of knowing how rapidly changes of molecular arrangement may occur after death. But, as Verworn says, "the biting sarcasm that Mephistopheles pours out before the scholar upon this practice of physiological chemistry must be quietly endured."

Although we do not know the nature of living matter—either in its simplest expression in the Protist gliding in the pond, or in its highest expression when its activity in our brains is associated with thought—we are not without data in regard to the sequence of vital processes. We can trace, by chemical analysis, at least some of the steps by which food is transformed until it becomes a usable part

* Prof. F. S. Lee's translation of Prof. Max Verworn's *General Physiology*, 1899, p. 117.

of the living body, and we can also trace some of the steps by which the waste products of activity are got rid of. Our position may be compared to that of visitors to the manufactory of some complex product: they see the raw materials coming in, they are allowed to follow the preliminary steps in their transformation; they see the final products passing out, and they are allowed to witness the process of "finishing" them; they see the rubbish that is cast away and are shown how some of the waste-products are re-utilised; but what they do not see is the gist of the whole business—the affairs of "the secret room"—where the essential transformations are kept secret.

Metabolism.—All theory apart, it is a fact of observation that there is in the living body a twofold process—of waste and of repair, of disruption and construction, of disassimilation and assimilation.

"One of the first to make this general idea more precise was De Blainville, who described vitality 'as a twofold internal movement of composition and decomposition.' At a later date, Claude Bernard, who may be called the pioneer of the 'protoplasmic movement,' distinguished 'disassimilating combustion and assimilating synthesis.' Of recent years various researches and speculations, especially those of Hering and of Gaskell, have led to yet more precise statements in regard to metabolism." *

Prof. Hering says: "Assimilation and disassimilation must be conceived as two closely interwoven processes, which constitute the metabolism (unknown to us in its intrinsic nature) of the living subtance, and are active in its smallest particles,—since living

* Thomson, *Science of Life*, p. 114.

matter is neither permanent nor quiescent, but is in more or less constant internal motion." In somewhat similar terms, Prof. Gaskell expounds the idea that life implies an alternation of two processes—one of them a running down or disruption (katabolism), the other a winding up or construction (anabolism).

THE UNSOLVED SECRET OF THE ORGANISM.

In the preceding portion of this chapter we have suggested the nature of the analysis by which the intact living creature has been, so to speak, taken to pieces, as one might do with a watch, and then theoretically reconstructed. Organism, organs, tissues, cells, protoplasm—these words express the various levels of analysis, and one result at least has been a greater precision of description, a more detailed and vivid picture of the facts of the case.

As the analysis has proceeded throughout the century, the enthusiasm of discovery has led again and again to a short-lived belief that a solution of the secret of the organism had been reached,—now as a system of correlated organs, or again as a city of co-operating cells. The discovery of the mainspring may be said to disclose the secret of the watch, and the discovery of the cylinder and piston may be said to disclose the secret of the steam-engine; and so it has seemed to some that the secret of the organism has been discovered in the combined functioning of the organs, in the combined properties of the tissues, in the combined changes of the cells, or in the metabolism of the protoplasm. But just as the mainspring's elasticity demands further analysis, and just as the change of water into expansive steam does not quite explain itself, so the biologists have sooner or later come to see that their presumed

explanations were in terms of things that required themselves to be explained.

For this reason, epoch after epoch, one "explanation" after another has been, so to speak, "found out," and there has been a recoil of caution or of disgust to the postulate of a specific "vital force," or to some other verbalism cloaking intellectual defeat.

To express the life of the organism in terms of its organs is no doubt a useful endeavour, so long as it is not forgotten that the functions of the organs—and, what is more, their correlated adaptations—remain a problem. To express the activity of the organs in terms of the activities of their component cells is an even more interesting task—useful and necessary like the previous step—yet surely in no sense an "explanation" as long as the life of the cell remains an unread riddle.

To some it has seemed for a brief moment that they saw the whole life of the organism clearly as comparable to an automatic, self-stoking, self-repairing heat-engine, or thermo-electric engine, or some unique combination of engines, but the vision has soon been obscured by the shadow of the thought that this marvellous engine grew into obvious complexity in a few days or months from a state of apparent simplicity, that it had the power of adjusting itself to more or less new conditions, and that it actually gave rise to other engines like itself, or that even a fragment of it reproduced the wonderful whole, and then has come the recoil to some subtle or crude theory of vitalism.

When the physiologist tries to express the function of an organ in terms of the activities of its cells he is really seeking a more thorough description,

and the search has been a fruitful one for physiology. The analysis is entirely consistent with scientific method and has been justified in its results. But the history of the enquiry reveals a twofold danger, (*a*) that the careless mistake a deeper description for an explanation, as if the cell and its protoplasm did not imply a mysterious microcosm, and (*b*) that in the analysis the unity of the organism be overlooked or slurred over as an unimportant fact.

But, it may be remarked, the physiologist has surely done more than analyse the organism into its component parts. Has he not summoned chemistry and physics to his aid, and shown that many phenomena which we call vital, which our predecessors attributed to the action of a special vital force, may be expressed in chemical and physical terms? Has he not interpreted by diffusion and osmosis the absorption of food from the alimentary canal and the interchange of gases which takes place in the lungs? Has he not given a physical account of the circulation of the blood and the ascent of sap? Has he not found the source of animal heat in the chemical changes which occur in the body-tissues, has he not artificially manufactured from simple substances various carbohydrates and the like, whose formation was previously believed to be inseparably associated with the controlling action of vital force? And thus we reach the position of those who say "that the further physiology advances, the more does it become possible to explain, on physical and chemical grounds, phenomena which have hitherto been regarded as associated with a special vital force; that it is only a question of time; that it will finally be shown that the whole process of life is only a more

complicated form of motion regulated solely by the laws which govern inorganic nature." *

What has been achieved is a detection of chemical and physical sequences in vital phenomena, what has not been achieved *as yet* is a redescription of a vital phenomenon in terms of chemistry and physics. Prof. J. T. Wilson states the case in an able address: †—"I shall not dispute the proposition that, in the progress of the science of physiology, physico-chemical theories of living processes have broken down all along the line. I readily admit that such theories have in every direction failed to accomplish that mechanical analysis of function which seemed to the physiologists of the later decades of the century to be so nearly within their grasp. Yet it would be grossly inaccurate to assert that the attempt to explain life as mechanism has resulted in nothing but failure. The fact is that mechanism after mechanism has been displayed, through the operation of whose chemical and physical properties the functional activity of the organism is subserved. On the other hand, it is true that the residual phenomena unexplained by these mechanisms may in a sense be held to embody the very essence of the mystery of organisation. It is not difficult to see that in the nature of the case this must be so. It is the penalty of the abstract character of the causal principle employed as the instrument of research. The forging of links in an endless chain of mechanical causation is a never-ending process,—the mys-

* G. Bunge, *Text-book of Physiological and Pathological Chemistry*, trans. by L. C. Wooldridge; London, 1890, p. 3. The quotation expresses the reverse of Bunge's own position.

† President's Address, *Proc. Linnæan Soc. N. S. Wales* XXIV., 1899, pp. 1–29.

tery ever recedes as we pursue it further into the recesses of organisation."

It may seem strange to ask whether the progress of nineteenth century physiology has been signalised by the achievement of re-expressing *any* vital phenomenon in terms of physics and chemistry. But it is, to say the least, very doubtful if there has been any such success. Leaving out of sight all phenomena, like the bursting of a dry pea-pod, or the projection of an image by the lens of the eye, which cannot be called vital, we press the question whether the contraction of a muscle or the movement of a sensitive plant, the flow of the blood or the ascent of sap, the respiratory changes in a lung or in a leaf, the absorption of food from the intestine or the formation of starch in a plant,—or *any* vital process can be completely described in chemical or physical terms. No doubt, chemical and physical processes have been detected, and have been followed out in some cases with great success, but has a complete redescription in chemical or physical terms ever been attained? "To me," Bunge says,* "the history of physiology teaches the exact opposite. I think the more thoroughly and conscientiously we endeavour to study biological problems, the more are we convinced that even those processes which we have already regarded as explicable by chemical and physical laws, are in reality infinitely more complex, and at present defy any attempt at a mechanical explanation."

Dr. J. S. Haldane goes even further:—"If we look at the phenomena which are capable of being stated, or explained in physico-chemical terms, we see at once that there is nothing in them characteristic of life. . . . We are now far more definitely

* *Op. cit.*, p. 3.

aware of the obstacles to any advance in this (physico-chemical) direction, and there is not the slightest indication that they will be removed, but rather that, with further increase of knowledge, and more refined methods of physical and chemical investigation, they will only appear more and more difficult to surmount. . . . All that is really shown by the partial success which has attended the application of physical and chemical principles of explanation in physiology is that in the course of investigation it is often possible to ignore for the time the distinctive features of life. For certain scientific purposes we may treat some part of the body as a mechanism, without taking into consideration the manner in which it is controlled and maintained; and in this way results of great value have been attained. But in doing all this we are deliberately ignoring or abstracting from all that is characteristic of life in the phenomena dealt with. The action of each bodily mechanism, the composition and structure of each organ, the intake and output of energy from the body, are all mutually determined and connected with one another in such a way as at once to distinguish a living organism from anything else. As this mutual determination is the characteristic mark of what is living, it cannot be ignored in the framing of fundamental working hypotheses."

We are lingering over this discussion because of its great historical interest. Again and again some success in discovering physico-chemical sequences in the living organism has awakened the expectation that the dawn of a mechanical theory (interpretation or re-description) of life was drawing nigh. Again and again the expectation has been disappointed, and the investigators have returned to rest in a

postulate of "vital force." This postulate is a vague one and its content has altered greatly even during the nineteenth century. For a time "vital force" was spoken of as a "hyper-mechanical" factor, a mystical power, a non-material agent, presiding over the activities of the body. But reason could not "repose on this pillow of obscure qualities," and the content of the postulate changed, for it is difficult to believe that Johannes Müller meant more by his vitalism than to express the fact that the physical and chemical processes in the living body are correlated in a manner which defies restatement in simpler terms. Many nowadays would agree with this or would advance to the more positive idealist position occupied by Bunge. This physiologist declares that "it would indeed be a lack of intelligence to expect with the senses to make discoveries in living nature of a different order to those revealed to us in inorganic nature," and yet he maintains "that all the processes of our organism capable of explanation on mechanical principles are as little to be regarded as vital phenomena as the rustling of leaves on a tree, or as the movement of the pollen when blown from stamen to pistil." In other words, he holds that the distinctively vital does not admit of mechanical restatement, and that light must come from above, not from below, i.e., from psychological rather than physical concepts.

Many other opinions of authoritative experts might be cited, varying greatly in their form, but with this common basis of agreement that the phenomena of life cannot be restated in the language of chemistry and physics. And yet, the reader may well ask, "Is this more than a pious opinion, an *argumentum ad ignorantiam?* Is not biological anal-

ysis still in its youth? Have not partial restatements been given of numerous functions? May one not look forward to the time when these may be completed?"

This leads us, in concluding this discussion, to follow Prof. Karl Pearson in pointing out again the radical misunderstanding which exists in many minds in regard to scientific method. The material of science is "the routine of our perceptual experience"; we think over this, though we never understand it; we make sure by experiment that the sequence of sense-impressions which constitutes the routine is not illusory; we make sure that the routine is perceived by others also (for science *is* social), lest we should be the victims of an idiosyncrasy; and by and by, if we are clever enough, we give "a description in conceptual shorthand (never the explanation) of the routine of our perceptual experience." "The problem of whether life is or is not a mechanism is thus not a question of whether the same things, 'matter' and 'force,' are or are not at the back of organic and inorganic phenomena—of what is at the back of either class of sense-impressions we know absolutely nothing—but of whether the conceptual shorthand of the physicist, his ideal world of ether, atom, and molecule, will or will not also suffice to describe the biologists' perceptions." That it does not at present seems the opinion of the more philosophical physiologists; if it ever should it would be "purely an economy of thought; it would provide the great advantages which flow from the use of one instead of two conceptual shorthands, but it would not 'explain' life any more than the law of gravitation explains the elliptic path of a planet."

"Atom" and "molecule" and the rest are concepts, not phenomenal existences, therefore even if the physicists' formulæ should fit vital phenomena —which they do not seem to do—there would be no "explanation" forthcoming, for "mechanism does not explain anything."

CHAPTER IX.

The Study of Structure.

THE MORPHOLOGICAL QUESTION AND ITS PROGRESSIVE ANSWERS.

One of the naturalist's first questions—however learnedly he may phrase it—is just one of the child's first questions, asked long before it can speak—"What is this?" In how many different tones—of fear, of awe, of wonder, of inquisitiveness—has this question been asked since man and science began! Was it not Aristotle's question when a new specimen was brought to him? was it not the question on the *Challenger* when the dredge came up? is it not the question on the lips of every teacher and student of natural history to-day?—What is this? It is a "simple question," but how hard to answer, as we press it further and further home, from external features to internal structure, from organs to tissues, from tissues to cells, as we put one lens after another in front of our own, as we call to our aid all sorts of devices—scalpel and forceps, razor and microtome, fixative and stain. "What is this," we say, "in itself and in all its parts? what is this by itself and when compared with its fellows and kindred?" and our answer broadens and deepens till it furnishes the raw materials of the science of Morphology.

The answer to the question: *What is this?* asked

again and again at different planes of analysis forms the raw material of morphology. This is the science of form and structure, just as physiology is the study of habit and function; the one has to do with the static, the other with the dynamic aspect of the organism. But the descriptive facts—the raw materials—do not constitute the science; the morphologist has to find unity amid manifoldness, to disclose the styles and principles of organic architecture, and to recreate the *Systema Naturæ*, not as a mere classification, but as the chart of history.

The history of morphology is, as Prof. Patrick Geddes points out, parallel to that of physiology. It is the history of a gradually deepening analysis.

(1) *The Organism.*—In early times, the answer to the question: *What is this?* was chiefly concerned with the external appearance of the intact creature, —its symmetry, shape, architectural plan, and the like, as is expressed in the work of men like Ray and Linnæus. Even at this level the morphologists' labours are not nearly completed. "Each new species described means a leaf added to Linné's *Systema Naturæ*." *

(2) *The Organs.*—The description of external characters is, however, only the beginning of morphology; an analysis of organs is the next step, which may be especially associated with the work of Cuvier as zoologist, of Jussieu as botanist, and of Goethe as both. This task is also an unending one, "to which every new descriptive anatomical research belongs as clearly as if it were published as an appendix to Cuvier's *Règne Animal*." *

(3) *The Tissues.*—The next logical step was

* P. Geddes, A synthetic outline of the history of biology, *Proc. Roy. Soc. Edin.*, 1885–1886, pp. 905–911.

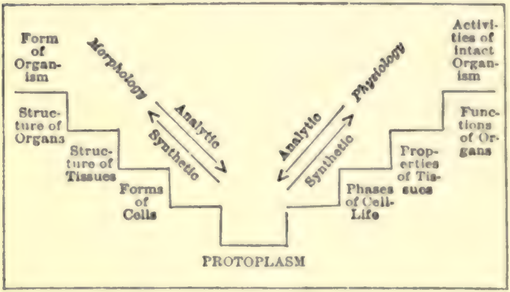
taken just at the dawn of the nineteenth century, when Bichat, in his *Anatomie Générale* (1801) analysed the body into its component tissues,—muscular nervous, glandular, connective, and so on. This may be called the beginning of histology, which has now so many devotees. From Bichat's classic we pass to Leydig's foundation of comparative histology (*Lehrbuch der Histologie des Menschen und der Thiere,* Frankfurt, 1857)—a most remarkable work for its date, and it brings us to the modern study of tissues, which has been so much stimulated by improvements in microscopic apparatus and technique. As the researches of Professor Albert von Kölliker of Würzburg extend over a period of sixty years, and over the entire field of animal histology, we could not choose a more fitting or more illustrious representative of nineteenth-century histological research.

(4) *The Cells.*—To the scalpel the lens was added; and then the scalpel was supplemented by the razor (first used by hand and now in a microtome); and lens was added to lens to form a compound microscope. Thus minute analysis could not remain long at the level of tissues; these were soon analysed into their component or originative cells,—the nucleated corpuscles of living matter which form the basis of all organic structure. This step must be especially associated with the work of Schleiden and Schwann, who formulated the "Cell-Theory" in 1838–39. With the study of cell-structure hundreds of modern workers are more or less exclusively occupied.

(5) *Protoplasm.*—The fifth and last step in morphological analysis, within the limits of biology, is that which passes from the cell as such to a study of the living matter and other substances which com-

pose it. With this, though it is difficult to select names, the work of Dujardin, Von Mohl, and Max Schultze may be associated.

This outline is based on the luminous but exceedingly short paper by Professor Patrick Geddes already referred to, and a fuller exposition will be found in the writer's *Science of Life* (1899). A diagrammatic summary may be useful.



It should be carefully noted that each step in analysis makes a corresponding step of synthetic reinterpretation possible. But the reconstructive process always lags far behind that of analysis.

In studying structure (morphology) the methods are—observation, analysis, and comparison. We begin with external form and symmetry, always harmonious and beautiful in a natural wild animal. We work with the scalpel till we see the creature through and through as if it were transparent; we persevere till we see it as a great city—usually far excelling any city of ours—with regions which we call organs, streets which we call tissues, houses

which we call cells. We get the help of microtome and microscope, of fixing and staining re-agents, and work on until we see the intricate structure of each house,—the furnishings and inhabitants of each cell. We try to work back again to the unity which we have taken to pieces; we compare organism with organism and detect their relationships; we compile a census and construct a genealogical tree.*

FOUNDATIONS OF MORPHOLOGY.

Although there were untiring and keen-sighted comparative anatomists in the eighteenth century—such as John Hunter and Vicq d'Azyr—the modern period may be fairly dated from the work of Cuvier and Goethe, who, though almost antithetic in their outlook on nature, may be called the joint-founders of comparative morphology.

To Georges Cuvier (1769–1832) the science owes much, not only for his rich accumulation of anatomical description, but for his attempt to give an anatomical basis to classification, for his appreciation of the value of fossils, and for his insistence on the correlation of parts. The idea expressed in the phrase "the correlation of parts" is now familiar:—the organism is no haphazard aggregate of characters, but a unified integrate. Part is bound to part, so that if the one varies the other varies with it. In short, "there are many members which are members one of another, in one body." It must be confessed, however, that Cuvier tended to exaggerate the value of his guiding principle, and that he did not appreciate its full significance as that has ap-

* See J. Arthur Thomson, *The Humane Study of Natural History*, *Humane Science Lectures*, Bell, London, 1897.

peared in post-Darwinian days. To Cuvier, who was an anti-evolutionist, the "correlation of parts" was simply a morphological fact.

We would place next the name of Goethe, not because of his anatomical discoveries, which were few in number, but because of the clearness with which his genius discerned and proclaimed "the fundamental idea of all morphology—the unity which underlies the multifarious varieties of organic form." *

The idea which was more or less clearly in the mind of Joachim Jung (1678) and of Linnæus (1760, 1763) that the appendicular organs (leaves, bracts, sepals, petals, etc.) arising from the stem of a flowering plant are all fundamentally the same leaf-organ in various forms, was rehabilitated and in part demonstrated by the embryologist Casper Friedrich Wolff (1767), who said "all parts of the plant, except the stem, are modified leaves," and by Goethe in his famous essay *Versuch die Metamorphose der Pflanzen zu erklären* (1790). It may be that the evidence Goethe gave of the fundamental unity of foliar and floral organs would not be considered conclusive nowadays, but his essay—published with some difficulty and for many years little noticed—is a famous document in the archives of botany, an early expression of an idea which has now saturated the whole science. The morphological equivalence of the appendicular organs is now universally admitted, though the direction in which the evolution has taken place—whether from foliage-leaf to reproductive-leaf (sporophyll) or *vice versa*—remains a subject of discussion.

Some years previously Goethe had made an-

* Geddes, Article Morphology, *Encyclopædia Britannica.*

other discovery, regarding which he wrote to Herder: —"I must hasten to tell you of a piece of good fortune that has happened to me. I have found —neither gold nor silver, but what gives me inexpressible delight—the intermaxillary bone in man." "I have such delight," he wrote to another, "*dass sich mir alle Eingeweide bewegen.*" The reason for his exuberant delight in proving the presence of this little bone in front of the upper jaw was due to his conviction of the unity of plan in vertebrate skeletons. That man had no intermaxillary had been regarded as a distinctive peculiarity; but Goethe was right in his conviction of the all-pervading similitude of structure between man and beast. While Goethe was quite independent in his discovery, it should be noted that the name of Vicq d'Azyr must also be associated with the bone in question.

The two discoveries which we have noticed remain as part of the framework of science, but the same cannot be said of Goethe's vertebral theory of the skull (which Oken also suggested). According to this theory, which Goethe arrived at partly from a study of the insect's body, evidently built up of a series of rings or segments, and partly from the sight of a crumbling sheep's skull which fell to pieces as he disinterred it, the skull is formed of six modified vertebræ.* The death-blow to this view, which prevailed for a long time, was given by Reichert and Ratke, Gegenbaur and Huxley, who showed that, although the head is built up of a series of segments, originally comparable to those of the trunk, this can-

* It is a strange historical fact that a sheep's skull on the Hartz Mountains led Oken to the same theory as the sheep's skull in the Jewish cemetery in Venice had suggested to Goethe.

not be said of the skull as such. At the same time, Goethe's theory was a keen-sighted morphological hypothesis, well worthy of being carefully tested.

We might also refer to Goethe's views on individuality, division of labour, correlation, adaptation, and the general doctrine of evolution; * but we have probably said enough to show why the poet-naturalist may be ranked among those who laid the foundations of morphology.

Lamarck was rather an evolutionist than a morphologist, but it must be remembered that in 1794 he drew with a firm hand the distinction, which Aristotle had hinted at, between vertebrate or backboned and invertebrate or backboneless animals. Although our knowledge of transitional forms, like Balanoglossus, not to speak of the Tunicates, has lessened the rigidity of Lamarck's line, the distinction is universally recognised as one of great practical convenience. Lamarck also defined a number of groups—Crustacea, Arachnida, and Annelida—which are still regarded as natural divisions, and he may be fairly called one of the founders of the comparative anatomy of invertebrates. The very antithesis of Cuvier, he allowed his evolutionary theory to colour his whole work.

Étienne Geoffroy Saint-Hilaire, author of the remarkable *Philosophic Anatomique* (1818–1823) in which he exaggerated the idea of "unity in organic structure," was another expert comparative anatomist who was profoundly influenced by the evolution-idea. Meckel, on the other hand, even more illustrious as an anatomist, was distinctly Cuvierian.

* See Prof. H. Reichenbach, *Goethe und die Biologie. Bericht Senckenberg Nat. Gesellschaft*, Frankfurt a. M., 1899, pp. 124–155.

Although Johannes Müller was probably greatest as a physiologist, he touched and influenced every department of biology, and his touch was that of genius. Even if he had left no record behind him but his work in comparative anatomy, his place on the roll of honour would be high. And apart from actual work, it should be recalled that Virchow, Kölliker, Gegenbaur, Haeckel, Brücke, Günther, and Helmholtz were among his pupils.

Sir Richard Owen (1804–1892) links Cuvier, at whose feet he sat for a short time, to Gegenbaur and Huxley, excelling Cuvier in the accuracy of his work and in the generalising spirit which he brought to bear upon his problems, but occupying a strange midway position,—on the one hand, extremely conservative and unappreciative of Darwinism; on the other hand, really believing in the derivation of species from one another.

Of the work of Owen and others we have elsewhere given a brief sketch,* and must be content here to emphasise the importance of the service which he rendered to morphology by his clear distinction between homologous and analogous organs.

Organs which resemble one another in essential structure and in development are called *homologous;* organs which resemble one another in the function they perform are called *analogous.* (*a*) Thus the wing of a flying bird is homologous with the arm of man; there is a fundamental similarity in the bones, muscles, nerves, and blood-vessels; they have also the same mode of development; both are true fore-limbs, but they are not analogous, for men do not fly, nor do birds grip with their fingers. (*b*) The

* Science of Life, 1899.

wing of a flying bird is analogous with that of a butterfly, for both are organs of true flight, which strike the air; but they are not homologous, for there is no resemblance in their structure or development. (*c*) Thirdly, the wing of a flying bird is both homologous and analogous with the wing of a bat.

It must not be supposed that the question is so easy as the illustrations given may suggest. Indeed there are few questions more difficult than the criteria of homology. But the importance of the distinction which Owen drew is obvious, for a true or natural classification which groups related forms together must be based on the demonstration of homologies. Perhaps the most important addition to what Owen said is due to Professor Ray Lankester who, in 1870, distinguished *homogeny* (correspondence due to common descent) from *homoplasty* (correspondence due to similar adaptations in unrelated forms).

Starting again from Goethe, we might, if space permitted, seek to show how the morphology of plants developed through the labours of Schleiden (1804–1881) the title of whose text-book (1842–43) *Botany as an Inductive Science* struck a new note, of Von Mohl, of Carl von Nägeli, of Hofmeister, who from 1849 onwards did for the pedigree of plants what Gegenbaur, Huxley, and others did for animals, of Robert Brown, Irmisch, Hanstein, Alex. Braun, and many more. From these through De Bary and Sachs, we pass naturally to the active botanical morphologists of to-day.

It may be more useful to try to illustrate some of the more general steps in the progress of morphology.

The first edition of Herbert Spencer's *Principles of Biology* and Ernst Haeckel's *Generelle Morpho-*

logie are classics of which the nineteenth century might have been prouder than it was. They are monumental attempts to systematise and clarify the general conceptions which underlie all biological thinking and research.

Let us take a simple illustration. We say that one animal is "higher" than another, what do we mean? Merely, that it is liker ourselves? Or is there more precision in our standard? The answer is to be found in the words "differentiation" and "integration"; the higher animal is more differentiated and more integrated than the lower. And what the two big words mean is made plain in the classics referred to.

The progress of the individual, and of the race, is from simplicity to complexity. When we think over the animal series we also notice that before definite nervous organs appear there is diffuse irritability, before definite muscular organs appear there is diffuse contractility, and so on. In other words, functions come before organs. The attainment of organs implies specialisation of parts, or concentration of functions in particular areas of the body.

Contrast a frog with *Hydra,* and one of the great facts about the evolution of organs is illustrated. Among the living units which make up a frog, there is much more division of labour than there is among those of *Hydra.* An excised representative sample of *Hydra* will reproduce the whole, but you cannot perform this experiment with the frog. Now, the structural result of this physiological division of labour is *differentiation.* The animal, or part of it, becomes more complex, more heterogeneous.

Contrast a bird and a sponge, and another great fact about the evolution of organs is illustrated.

The bird is more of a unity than a sponge; its parts are more closely knit together and more adequately subordinated to the life of the whole. We call this kind of progress *integration*. Differentiation involves the acquisition of new parts and powers, these are consolidated and harmonised as the animal becomes more integrated.*

Stephenson's "Puffing Billy" was a lower organism than a locomotive of 1901; it showed less complexity of usefully functional parts, and it was less under unified control.

Our point is that we are continually using words like "organism," "development," "differentiation," "integration," "individuality," "character," "adaptation," and so on,—using them lightly as if there were no difficulties hidden in them--and that therefore such general philosophic works as the two we have named are of great value in expressing at least an attempt to criticise and clarify the categories which even the purest of "pure anatomists" must use in spite of himself. Neither Spencer nor Haeckel would regard his masterpiece of 1866 as final; indeed Spencer in his last years began to re-edit *The Principles of Biology;* and it is plain that the criticism of categories must develop as the science does, but the fact remains that there are few biological books of more recent date which come near those of Spencer and Haeckel in extent or lucidity of outlook.

Change of Function.—Division of labour involves restriction of functions in the several parts of an animal, and no higher animals could have arisen if all the cells had remained with the many-sided qualities of Amœbæ. Yet we must avoid thinking

* See the writer's *Outlines of Zoology*, 3rd edition, 1899.

about organs as if they were necessarily active in one way only. For many organs, e.g., the liver, have several very distinct functions, and we know how wondrously diverse are the activities in our brains. In addition to the main function of an organ there are often secondary functions; thus, the wings of an insect may be respiratory as well as locomotor, and part of the food canal of ascidians and lancelets is almost wholly subservient to respiration. Moreover, in organs which are not very highly specialised, it seems as if the component elements retained a considerable degree of individuality, so that in course of time what was a secondary function may become the primary one. Thus Dohrn, who has especially emphasised the idea of function change, says: "Every function is the resultant of several components, of which one is the chief or primary function, while the others are subsidiary or secondary. The diminution of the chief function and the accession of a secondary function changes the total function; the secondary function becomes gradually the chief one; the result is the modification of the organ." We may notice, in illustration, how the structure known as the allantois is an unimportant bladder in the frog, while in Birds and Reptiles it forms a fœtal membrane (chiefly respiratory) around the embryo, and in most Mammals forms part of the placenta which effects nutritive connection between offspring and mother.

Substitution of Organs.—The idea of several changes of function in the evolution of an organ, suggests another of not less importance which has been emphasised by Kleinenberg. An illustration will explain it. In the early stages of all vertebrate embryos, the supporting axial skeleton is the noto-

chord,—a rod developed along the dorsal wall of the gut. From Fishes onwards, this embryonic axis is gradually replaced in development by the vertebral column or backbone; the notochord does not become the backbone, but is replaced by it. It is a temporary structure, around which the vertebral column is constructed, as a tall chimney may be built around an internal scaffolding of wood. Yet, it remains as the sole axial skeleton in *Amphioxus,* likewise in great part in hag and lamprey, but becomes less and less persistent in Fishes and higher vertebrates, as its substitute, the backbone, develops more perfectly. Now, what is the relation between the notochord and its substitute, the backbone, seeing that the former does not become the latter? Kleinenberg's suggestion is that the notochord supplies the stimulus, the necessary condition, for the formation of the backbone. Of course, we require to know more about the way in which an old-fashioned structure may stimulate the growth of its future substitute, but the general idea of one organ leading on to another is suggestive. It is consistent with our general conception of development—that each stage supplies the necessary stimulus for the next step; it also helps us to understand more clearly how new structures, too incipient to be of use, may persist.

Rudimentary Organs.—In many animals there are structures which attain no complete development, which are rudimentary in comparison with those of related forms, and seem retrogressive when compared with their promise in embryonic life. But it is necessary to distinguish various kinds of rudimentary structures. (*a*) As a pathological variation, probably due to some germinal defect, or to the insufficient nutrition of the embryo, the heart of a mammal is

sometimes incompletely formed. Other organs may be similarly spoilt in the making. They illustrate *arrested development*. (*b*) Some animals lose, in the course of their life, some of the promiseful characteristics of their larval life; thus parasitic crustaceans at first free-living, and sessile sea-squirts at first free-swimming, always undergo *degeneration*. The retrogression can be seen in each lifetime. But the little Kiwi of New Zealand, with mere apologies for wings, and many cave fishes and cave crustaceans with slight hints of eyes, illustrate degeneration which has taken such a hold of the animals that the young stages also are degenerate. The retrogression cannot be seen in each lifetime, evident as it is when we compare these degenerate forms with their ancestral ideal. (*c*) But among "rudimentary organs" we also include structures somewhat different, e.g., the gill clefts which persist in embryonic reptiles, birds, and mammals, though they serve no obvious purpose, or the embryonic teeth of whalebone whales. These are "*vestigial structures*," traces of ancestral history and intelligible on no other theory. The gill clefts are used for respiration in all vertebrates below reptiles; the ancestors of whalebone whales doubtless had functional teeth. In regard to these persistent vestigial structures, it must also be recognised that we are not warranted in calling them useless. Though they themselves are not functional, they may sometimes be, as Kleinenberg suggests, necessary for the growth of other structures which are useful.

The foundations of comparative anatomy were laid by Cuvier. But the historical lineage shows the influence of another strain, that of the evolutionary anatomists, like Goethe and Étienne Geoffroy St.-

Hilaire. From these, as well as from Cuvier, there is, through Owen as a transition-type, an affiliation with more modern morphologists like Gegenbaur and Huxley, Lankester and Cope. Starting again from Goethe there has been an evolution of botanical morphologists, through Schleiden to Hofmeister, thence to De Bary and Sachs, and onwards to Goebel, Bower, Campbell, and others. But the development of general ideas of homology, differentiation, integration, substitution of organs, and the like has not been less important.

THE APPRECIATION OF FOSSILS.

When natural science was young, fossils had been regarded as "sports of nature" of a mineral sort, as still-born expressions of the earth's maternal virtue, as victims of the Noachian flood, and so on. The artist and thinker Leonardo da Vinci (born 1452) did indeed maintain that fossils were what they seemed to be—remains of animals that had once lived; Bernard Palissy (1580) a century later, was of the same opinion; and Steno, a Danish professor in Padua, was equally shrewd. Thus, through Martin Lister, contemporary with Ray, we reach the beginning of the nineteenth century when the foundations of palæontology were laid by Smith, Cuvier, Lamarck, and Brongniart. The word palæontology, like the idea which it expresses, is quite modern. Ducrotay de Blainville and Fischer von Waldheim seem to have been responsible for the term (about 1830), and it soon afterwards became a household word in science.

But although Smith, Cuvier, Lamarck, and Alex. Brongniart laid the foundations and made it impos-

sible to ignore the value of fossils as indices of the geological age and succession of strata, it was not till long afterwards that it became a general commonplace that palæontology was part of zoology and botany. To Huxley in particular we are indebted for the conviction that the study of an animal living to-day and of one living a million years ago, differ only as regards the method of preservation and examination.

As one of the most illustrious of British palæontologists—Dr. R. H. Traquair—has said: * "Palæontology, however valuable, nay, indispensable, its bearings on Geology may be, is in its own essence a part of Biology, and its facts and its teachings must not be overlooked by those who would pursue the study of Organic Morphology on a truly comprehensive and scientific basis. . . . Does an animal cease to be an animal because it is preserved in stone instead of spirits? Is a skeleton any the less a skeleton because it has been excavated from the rock, instead of prepared in a macerating trough? . . . Do animals, because they have been extinct for it may be millions of years, thereby give up their place in the great chain of organic being, or do they cease to be of any importance to the evolutionist because their soft tissues, now no longer existing, cannot be imbedded in paraffine and cut with a Cambridge microtome?"

That Palæontology is Biology and that Biology includes Palæontology is now admitted by all (as a theoretical proposition at least), but the recognition has been an important result of nineteenth-century work. The only hindrance to the practical recognition of the unity is that the correct interpretation

* Address Zoological Section, *Rep. Brit. Ass.*, Bradford, 1900.

of fossil remains often demands, e.g., in the case of fishes, a prolonged special training. "The nature of the remains with which the palæontologist has to deal renders their interpretation a task of so different a character from that allotted to the investigation of the structure and development of recent forms" * that the necessary division of labour tends to be exaggerated. Of the founders of palæontology three were on the whole biological,—Cuvier (Tertiary mammals), Lamarck (Molluscs), and Brongniart (Plants), while William Smith was mainly interested in the relation of the fossils to stratigraphical problems.

The palæontological work of the nineteenth century has been marked by several different kinds of achievements:—the compilation of a descriptive census of the extinct, the anatomical study of lost races, i.e., of those with no living representatives, nor, so far as we know, direct descendants, the discovery of missing links, and the working out of pedigree-lines in particular groups.

Study of Lost Races.—In studying fossils a distinction must be drawn between (*a*) those which are in no sense extinct, being represented to-day by living forms, e.g., *Lingula, Estheria, Ceratodus,* (*b*) those which, though forming extinct species, are represented to-day by living descendants, as is true of a very large number, and (*c*) those which are without known living descendants, which we must therefore call extinct types or lost races, e.g., Graptolites and Trilobites, Eurypterids and Pterodactyls. It is indeed a distinction of degrees, and more degrees might be recognised, but it is plain that the

* Traquair, *loc. cit.*

student of the wholly extinct Graptolites has no clue such as he has who studies fossil corals. Yet the study of these lost races is of profound interest, since they must be fitted into their appropriate place in the general scheme of zoological or botanical classification.

The famous French palæontologist, Albert Gaudry, has spoken thus of the extinction of races: "A host of creatures have vanished; the most powerful, the most fertile have not been spared. There is a sadness in the spectacle of so many inexplicable losses." Let us linger for a little over the fact—the details of which have been accumulated with consummate patience through the past century.

It seems clear from the rock-record that sudden disappearance has been very rare. The American bison's practical extermination in a few years is without parallel in pre-human days. Races waned and died out, but were not suddenly extinguished. They did not come to a catastrophic end. Another striking fact is that while evidences of senility have been detected in some of the last representatives of dwindling races, there are many cases where a full stop seems to have been put to the history of a stock while it was still in its prime. Nor is there any reason to speak of an elimination of weaklings; as Gaudry says: "While insignificant creatures persist, the princes of the animal world vanish—without return."

The problem of the causes which led to the extinction of races has been left by the nineteenth century unsolved. It is easy enough to refer to changes of environment for which the plasticity of the organism was insufficient, or to the struggle for existence between cuttlefishes and trilobites, between ichthyo-

saurians and cuttlefishes, or to constitutional defects, such as Lucretius thought of when he pictured races going down to destruction "hampered all in their own death-bringing shackles," or to other more or less plausible reasons, but the suggestions remain very vague and unsatisfactory.

Against the puzzling facts of extinction, we have to place the grander fact that, in spite of all, life has been slowly creeping upwards. We may quote a paragraph—freely translated from Gaudry's *Enchainements du Monde animal dans les temps géologiques* (1878–1896).

"The organic world taken as a whole has made progress. Suppose a voyager on the oceans of ages; in the Cambrian times his barque meets trilobites, but no fishes; he nears the shore and there is the silence of death. After long voyaging he finds himself at the end of the primary era, fishes have replaced trilobites, and on land there is no longer silence: there is the tramp and cry of reptiles who prophesy the advent of warm-blooded vertebrates. The traveller sails from age to age and reaches the middle of the secondary era. Charmingly beautiful ammonites play around his vessel, legions of belemnites mingle with them; ichthyosaurs, plesiosaurs, and teleosaurs follow his track. He goes ashore, and the giant dinosaurs resting on their tails open their huge arms; pterodactyls and other dragons swoop aloft; the first bird tries its wings, and some small mammals show face timidly. Nature, marvellous in the primary ages, has become yet more marvellous; it has made progress. If our traveller be not fatigued with his long wanderings, he will find in the Tertiary ages the first monkeys, and horses, and a thousand other mammals. Later on he will find himself—the man—artist and poet—minister and interpreter of

nature—the man who thinks and prays. Truly the history of the world as a whole is the history of a progressive development. Where will this development lead us?"

Discovery of Missing Links.—In trying to reconstruct the pedigree of a race reliance is placed on three sets of facts,—(*a*) the grades of structure exhibited among the living representatives, (*b*) the steps in individual development, and (*c*) the evidence of the race's history as found in the fossils of successive ages. The third method is the most direct, and if the rock-record were complete, the facts of the history of life would be clear.

The fossil-containing rocks have often been compared to a library, with the oldest books on the lowest shelves, but what a library! Spoilt by fire, by water, by earthquake, by decay; here half a shelf a-wanting and there a series of volumes with most disappointing gaps; pages out of books, words missing in sentences, and the vowels a-wanting like the points in Hebrew. One is troubled also by palimpsests, one record on the top of another.

It is important to realise this from the study of strata, since there are still ill-natured people who suggest that evolutionists simply take refuge in "the imperfection of the geological record," when they are getting the worst of an argument. The imperfection is a lamentable fact, and we cannot wonder at it when we remember how young man is—his whole history but a tick of the geological clock; when we notice that many areas are still unexplored, and that much ground—being covered by sea—must remain unknown; when we understand that only hard organisms or hard parts are likely to be preserved,

that only certain rocks are suitable for preserving their enclosures, and that many rocks have been unmade and remade in the course of ages. As we walk along the shore and study the jetsam, we see how quickly many of the sea's memoranda are lost.

On the other hand, we must not exaggerate the imperfection; indeed, the biologist has often much reason to be gratefully surprised at the reverse. Many fossil jelly-fishes—most unlikely subjects of preservation—are known, and have been carefully studied, e.g., by Haeckel and by Walcott. Sometimes a whole series can be followed, and the transitions from species to species studied, as in the case of fresh-water beds containing shells of *Paludina* and *Planorbis.* On a larger scale, Hyatt's tracking of the evolutionary paths of the Ammonites is a monumental piece of work. In some cases, even in Graptolites, a little palæontological embryology, or study of young forms at least, is possible. Half a dozen unborn young may be seen inside an ichthyosaurus in the museum at Stuttgart and the remains of belemnites may be counted in the stomach. Sometimes in a fossil fish there is not a bone a-missing or out of place, though very much the reverse is the rule.

It is difficult to have much satisfaction in the fragmentary remains (skull-cap, femur, and two teeth) of *Pithecanthropus erectus* found by Dubois (1894) in what were regarded as Upper Pliocene deposits in Java. The remains *may* be those of a transitional form between man and his unknown simian ancestors, but the evidence is by no means sufficient. But, in other cases, the preservation is so perfect that certain conclusions may be arrived at. The skeleton of *Phenacodus,* carefully studied

by Cope and Osborn, is certainly that of an old-fashioned Ungulate, with some affinities to other stocks, and perhaps one of the earliest ancestors of the horse. The skeleton of *Archæopteryx,* in the lithographic slates of Solenhofen, carefully studied by Dames and others, is certainly that of a bird with more distinctly reptilian affinities than any living form shows. The skeleton of *Palæospondylus,* from the Devonian of Caithness, discovered and described by Traquair, is certainly that of a tiny primitive vertebrate, whose *real* reconstruction from many specimens has been a triumph of palæontological skill. And thus we might continue, for nineteenth-century palæontology has made it abundantly clear that links are not always missing. It would be absurdly pessimistic to suppose that there are not many still awaiting discovery.

Evolutionary Palæontology.—The doctrines of the Cuvierian school dominated most of the palæontological work of the first half of the nineteenth century. The work of Owen, Louis Agassiz, and Bronn was in some respects transitional, for though none was a thorough-going evolutionist, they all had an idea of progressive development. The dawn of evolutionary palæontology practically dates from Darwin's *Origin of Species* (1859), and now it may be said that almost all palæontologists are keen evolutionists.

Von Zittel says:—"To determine the genetic relationships, the ancestry, the modification, and the further development, in short the race-history or phylogeny, of the organisms under consideration is now regarded as the essential, by many, indeed, as the chief aim of palæontology."

Traquair says:—"From the nature of things it

is clear that the voice of the palæontologist can only be heard on the morphological aspect of the question (factors of evolution), but to many of us, including myself, the morphological argument is so convincing that we believe that even if the Darwinian theory were proved to-morrow to be utterly baseless, the Doctrine of Descent would not be in the slightest degree affected, but would continue to have as firm a hold on our minds as before." Thus he took for the theme of his Presidential Address to the zoological section of the British Association in 1900, the palæontological evidence of Descent in the case of fishes.

Marsh said:—"This revolution has influenced palæontology as extensively as any other department of science, and hence the new period. . . . To-day, the animals and plants now living are believed to be genetically connected with those of the distant past; and the palæontologist no longer deems species of the first importance, but seeks for relationships and genealogies connecting the past with the present."

The appreciation of the true nature of fossils, the recognition of palæontology as biological, the compilation of great censuses of the extinct, the study of lost races, the discovery of missing links, and the adoption of the evolutionary outlook in palæontology, are among the great steps in the morphological progress of the nineteenth century.

MINUTE ANALYSIS.

One of the clearest illustrations of the influence of improvements in instruments on the progress of

theoretical science is that afforded by the results which have come to biology through the perfection of the microscope. In no case has an instrument contributed more to the deepening of a science.

It is hardly necessary to point out that the magnification of an object does not necessarily mean a better understanding of it, and it must be admitted that there are many results of microscopic analysis which have complexified problems without helping towards their solution; but the historical fact is certain that microscopic analysis has made *many* biological problems clearer, and has saved us from supposing that the apparent simplicity of others is real.

Invention of the Compound Microscope.—As distinguished from a mere magnifying lens, the microscope is about three centuries old. There is strong evidence that the compound microscope was invented by Galilei about 1610, but there is also evidence in favour of giving credit to Hans and Zacharias Janssen, spectacle-makers of Middelburg in Holland, who are said to have made a compound microscope sometime between 1590 and 1609. Huyghens and others have claimed the discovery for Cornelius Drebbel, a Dutchman, about the year 1621, and Fontana, a Neapolitan, claimed that he had made a compound microscope in 1618. The case for Galilei seems, on the whole, strongest; but it is probably impossible now to decide with certainty.*

Early Microscopists.—Although many of those who first used the microscope did little more than accumulate magnifications, we must look back grate-

* See Mayall, *Lectures on the Microscope*, London, 1886. *The Microscope*, Carpenter and Dallinger, London, 1891.

fully to the pioneers who began the minute analysis so characteristic of the nineteenth century. Keen sighted observers like Leeuwenhoek, Malpighi, Hooke, and Grew, in the second half of the seventeenth century were the forerunners of modern histology. When Leeuwenhoek demonstrated unicellular organisms to the then young Royal Society of London, whose members (present at the meeting) signed an affidavit that they had really seen the minute creatures in question, a vista was opened which is still widening before us after the lapse of more than two centuries.

Steps towards the Cell-Doctrine.—The word "cell" (an unfortunate one at the best) was first used in histological description by Hooke (1665) and Grew (1671–75), but not in a very accurate or definite way. Malpighi (1675) also described minute "utricles," *some* of which we should now call cells.

Leeuwenhoek (Phil. Trans. 1674) seems, as we have noted above, to have been the first to describe single-celled organisms. But the hint was not quickly followed, for it was not till 1755 that Rösel von Rosenhof described the Amœba or "Proteus animalcule."

In his *Theoria Generationis* (1759) Caspar Friedrich Wolff recognised the "spheres" and "vesicles" composing the embryos of plants and animals. But he did not discern their nature or their importance.

In 1784, Fontana discovered the kernel or nucleus of the cell which we now know to be essential to the vitality of any ordinary protoplasmic unit. But he did not know the importance of his discovery,

and had not the least idea that the little spot he observed was a most intricate structure.

The fact that Bichat, in his *Anatomie Générale* (1801), speaks only of tissues, shows that the import of cells was not realised at the beginning of the nineteenth century. Little importance can be attached to the "vesicles" and "Urschleim" which Oken discussed in 1805, for this illustrious representative of the "Naturphilosophie" did not concern himself much with concrete details. The observations of Mirbel on the structure of embryos had more objective worth.

"A still closer approximation to the truth is found in the works of Turpin (1826), Meyen (1830), Raspail (1831), and Dutrochet (1837); but these, like others of the same period, only paved the way for the real founders of the cell-theory." *

In the first volume of his epoch-making work on the development of animals (1828), Karl Ernst von Baer "made the following prophetic statement":—"Perhaps all animals are alike, and nothing but hollow globes at their earliest developmental beginning. The farther back we trace their development, the more resemblance we find in the most different creatures. And thus to the question whether at the beginning of their development all animals are alike, and referable to one common ancestral form, considering that the germ (which at a certain stage appears in the shape of a hollow globe or bag) is the undeveloped animal itself, we are not without reason for assuming that the common fundamental form is

* Prof. E. B. Wilson, *The Cell in Development and Inheritance*, 2d ed., 1900, p. 2.

that of a simple vesicle, from which every animal is evolved, not only theoretically, but historically." * Considering the date we cannot regard the statement as other than a marvellous premonition.

In 1835, Robert Brown showed that a nucleus was normally present in all vegetable cells, thus raising Fontana's discovery to a higher level of importance. And, in the same year, Johannes Müller made a definite comparison between the cells of plants and those of the notochord in animals,—the beginning of a recognition of the fundamental unity of vegetable and animal structure. The observations of Dujardin, Purkinje, Von Mohl, Valentin, Unger, Nägeli, Hofmeister, Henle, and many others might also be alluded to.

This is no complete history, but we have cited enough to show how very gradually the way was prepared for the formulation of the cell-doctrine by Schwann and Schleiden in 1838–39.† "The "significance of Schleiden's, and especially of "Schwann's, work lies in the thorough and compre-"hensive way in which the problem was studied, "the philosophic breadth with which the conclusions "were developed, and the far-reaching influence "which they exercised upon subsequent research." In this respect it is hardly too much to compare the *Mikroskopische Untersuchungen* with the *Origin of Species.*

* Cited from Dr. Hans Gadow's notes to Haeckel's *Last Link*, 1898.

† Sir William Turner, "The Cell Theory, Past and Present," Inaug. Address Scottish Microsc. Soc., 1890, and in *Nature*, 1890; Prof. J. G. McKendrick, "On the Modern Cell Theory" (Proc. Phil. Soc., Glasgow, 1888), and in his text-book of Physiology; P. Geddes, articles *Morphology* and *Protoplasm*, *Encyclopædia Britannica*.

The cell-doctrine has been already stated; in its morphological aspect it emphasises the fundamental unity of minute structure in all living creatures. The simplest organisms are single cells. All other organisms are built up of many cells or modifications of cells. Among themselves they show division of labour which is expressed in the great variety of form and structural detail. From the fertilised ovum onwards, the formation and growth of the body is due to cell-division. This occurs in various fashions, but especially in one complex (indirect or karyokinetic) fashion which shows a fundamental similarity throughout the entire series.

Corroborations of the analysis into cells were rapidly forthcoming. As early as 1824, Prévost and Dumas had studied the cleavage of the fertilised ovum, and it may be noted that some stages of this can be seen with the naked eye in the relatively large egg of the frog, which measures about one-tenth inch in diameter. Similarly, Martin Barry (1838–41), Reichert (1840), Henle (1841), Kölliker (1843–46), and Remak (1841–52) showed how the cells of the embryo arise from the division of the fertilised egg cell.

Moreover, Goodsir in 1845, Virchow in 1858, proved that in all cases, pathological as well as normal, cells arise from pre-existing cells, that *omnis cellula e cellula* is a general fact of histology.

There was a strong tendency, however, to attach too much importance to the cell wall, and too little to the contained cell substance. The all-important protoplasm was not adequately appreciated.

In 1835, Dujardin described the "sarcode" of Protozoa, and other animal cells; in 1839, Purkinje compared the substance of the animal embryo (which

he was the first to call "protoplasm") with the "cambium" of plant cells; in 1846 Von Mohl emphasised the importance of the "protoplasm" in vegetable cells; Ecker (1849) compared the contractile substances of muscles with the living matter of Amœbæ; Donders also referred the contractility from the wall to the contents; Cohn suspected that the "sarcode" of animals and the "protoplasm" of plants must be "in the highest degree analogous substances"; and finally, Max Schultze (1861) accepted the growing belief that plants and animals were made of very similar living matter, and defined the cell as a unit mass of nucleated protoplasm.*

"The full physiological significance of protoplasm, its identity with the 'sarcode' of the unicellular forms, and its essential similarity in plants and animals, was first clearly placed in evidence through the classical works of Max Schultze and De Bary, beside which should be placed the earlier works of Dujardin, Unger, Nägeli, and Mohl, and that of Cohn, Huxley, Virchow, Leydig, Brücke, Kühne, and Beale." †

Louis Agassiz, not being an evolutionist, spoke of the cell-doctrine as "the greatest discovery in the natural sciences in modern times"; and, apart from the idea of evolution, it may be called the most influential. For it is important to notice that it has not only affected the analysis of the anatomist and the physiologist, and the whole of embryology, but has entirely changed our position in regard to some

* See the writer's *Outlines of Zoology*, Introduction.

† E. B. Wilson, *The Cell in Development and Inheritance*, p. 5.

of the general problems of biology, notably in regard to heredity and inheritance.

The student who wishes to understand the position of cellular biology at the beginning of the twentieth century should read a luminous book by Prof. E. B. Wilson (*The Cell in Development and Inheritance*, 2nd ed., 1900), along with which we may cite Delage's *La structure du protoplasma et les théories sur l'hérédité et les grands problèmes de la biologie générale* (1895). From Wilson's work, we venture to quote the first paragraph:—

"During the half-century that has elapsed since the enunciation of the cell-theory by Schleiden and Schwann, in 1838–39, it has become ever more clearly apparent that the key to all ultimate biological problems must, in the last analysis, be sought in the cell. It was the cell-theory that first brought the structure of plants and animals under one point of view, by revealing their common plan of organisation. It was through the cell-theory that Kölliker, Remak, Nägeli, and Hofmeister opened the way to an understanding of the nature of embryological development, and the law of genetic continuity lying at the basis of inheritance. It was the cell-theory again which, in the hands of Goodsir, Virchow, and Max Schultze, inaugurated a new era in the history of physiology and pathology, by showing that all the various functions of the body, in health and in disease, are but the outward expression of cell-activities. And at a still later day it was through the cell-theory that Hertwig, Fol, Van Beneden, and Strasburger solved the long-standing riddle of the fertilisation of the egg and the mechanism of hereditary transmission. *No other biological generalisation, save only the theory of organic evolution, has brought so many apparently diverse phenomena under a common point of view or has accomplished more for the unification of knowledge. The cell-theory must there-*

fore be placed beside the evolution-theory as one of the foundation-stones of modern biology."

The progress of cellular biology or cytology since the formulation of the cell-doctrine has been along several different lines, connected of course by side branches.

(*a*) *The complexity of cell-structure* has become more and more apparent. It includes many components,—the general cell-substance or cytoplasm, the nucleus with its readily stainable "chromatin" and illusive unstainable "achromatin," the centrosomes (present in the majority of animal-cells) which play an important part in division, the cell-wall or the cell-margin which shows many degrees of differentiation, the intercellular bridges which in many cases bind one cell to another, and so on. The cell is a little world of extraordinary complexity, as the work of Auerbach, Bütschli, Carnoy, Flemming, Fol, Guignard, Hertwig, Strasburger, Van Beneden, and a score of other prominent workers has shown.

(*b*) The same impression of a progressive revelation of complexity is afforded if we consider any particular component of the cell, such as the nucleus, or the system of radiating filaments which form a halo round the centrosome, or the structure of a vibratile lash or cilium, or the general cell-substance. In regard to the last, some, like Frommann and Arnold, have described an intricate network; others, like Flemming, a tangled coil of fibrils; others, like Altmann, a crowd of granules in a gelatinous matrix; and others, like Bütschli, a fine alveolar or vacuolar appearance like that of an emulsion. It seems probable that the minute structure of cell-sub-

stance varies in different cells and even at different times within the same cells. The investigations of Bütschli, who has studied the structure of fine artificial emulsions and compared this with that of cells both fixed and living, who has also investigated the fine structure of dead organic substances like cellulose, starch grains, chitinous shells, spicules, etc., mark at present the extreme of microscopical analysis. It is interesting to note that all his results favour the interpretation that the complexity is alveolar or vacuolar like that of a very delicate emulsion. Better lenses, thinner sections, differential staining, and other improvements in technique have led to the disclosure of a complexity undreamt of half a century ago. The contrast between the modern analysis of a spermatozoon or of a cilium and that of even a quarter of a century ago is most vividly illustrative of the increased precision. If any one name may be associated with the recognition of complex cellular organisation, it should be that of Brücke, whose classic work entitled *Die Elementarorganismen* was published in 1861. But even if we have succeeded, at length, in getting down to the ultimate elements of living matter, or "idiosomes," in which some believe that the secret of organisation, growth, and development lies hidden, we have to hand on the problem of their nature to the twentieth century still unsolved. "What these idiosomes are, and how they determine organisation, form, and differentiation, is the problem of problems on which we must wait for more light. All growth, assimilation, reproduction, and regeneration may be supposed to have their seats in these fundamental elements. They make up all living matter, are the

bearers of heredity, and the real builders of the organism." This deliverance is quoted from an essay by Prof. C. O. Whitman, one of the modern leaders, but it will be observed that it leaves the riddle of organisation unread.

(*c*) An exceedingly important step was made when it was made clear that new cells arise from the division of pre-existing cells,—a step which may be particularly associated with the names of Goodsir (1845) and Virchow (1858). Of great importance also was the general rationale of cell-division, which seems to have been suggested independently by R. Leuckart, Herbert Spencer, and Alexander James; it is often referred to as the Leuckart-Spencer principle, and is based on the fact that in cell-growth the increase of mass or volume outruns the increase of surface. When the cell has, let us say, quadrupled its original mass by growth, it has by no means quadrupled its surface (the former increasing in spherical cells as the cube, the latter as the square, of the radius); physiological difficulties set in, and at "the limit of growth" the cell divides, halving its mass, and gaining new surface.* But attention has been mainly concentrated on the details of the actual process of cell-division, which is due, as Prof. Wilson says, to "the co-ordinate play of an extremely complex system of forces." Its necessity is clear (on the Leuckart-Spencer principle) as the only feasible mode of growth; its end is clear—to divide the essentials of the mother-cell equally between the daughter-cells; but, in spite of continuous attempts, the actual mechanism of the process remains obscure. Three results seem clear:—(*a*) the fundamental

* See the writer's *The Science of Life*, p. 108.

similarity of process and result in spite of many peculiarities in individual cases, (*b*) the occurrence of complex tensions, strains, and stresses in the process, and (*c*) the impossibility (at present) of any mechanical interpretation.

(*d*) Various facts, such as the multiplication of nuclei in embryos without corresponding cell-delimitation, and the influence that the growth of the mass has upon the forms of cell-division which follow, have led many to add saving-clauses to the cell-theory, as Sachs did when he said "cell-formation is merely one of the numerous expressions of the formative forces which reside in all matter, in the highest degree, however, in organic substance"; or as De Bary did when he said, "That the plant forms cells is more accurate than the statement that cells form plants." "*Die Pflanze bildet Zellen, nicht die Zelle bildet Pflanzen.*" In short, the conception of the cell has to change with increasing knowledge of its nature and origin; though it may be still defined as a protoplasmic area in which nucleoplasm and cytoplasm are combined in a unified life.

(*e*) Though it is not exactly relevant in this chapter, we must note the gradually increasing body of facts which inform us as to the physiological relations of the individual cell to its environment (of physical and chemical influences, and of its fellows). The bulk of Davenport's *Physiological Morphology* is occupied with a discussion of this problem.

(*f*) Finally, the progress of cytology has had its influence on that study of Bacteria and other micro-organisms which has been one of the features of the latter part of the nineteenth century. The door which Leeuwenhoek opened in the seventeenth cen-

tury remained merely ajar till after the cell-theory had been formulated. Since then the study of unicellular plants and animals has been eagerly pursued. From Dujardin and Ehrenberg to Haeckel and Bütschli for Protozoa,—from Pringsheim and Cohn to De Bary for Protophytes, there was a continuous study of the simplest forms of life, and there are many to-day who devote themselves to this study and maintain that it is still only beginning. In connection with bacteriology the names of Pasteur and De Bary, Lister and Koch, Duclaux and Roux, deserve particular mention.

CHAPTER X.

GENEOLOGICAL.

GENEOLOGY.

A TERM is needed for the study of living creatures in their time-relations, for the enquiry into their individual development, racial evolution, and historical aspects generally; and we have suggested the term *geneology* (changing a letter in the narrower word genealogy). This "science of becoming" would include (*a*) individual development, growth, and life-history (ontogeny); (*b*) the racial history (phylogeny); (*c*) the relation of genetic continuity between successive generations (heredity).

DEVELOPMENT OF THE INDIVIDUAL.

Beginnings of Embryology.—Embryology is entirely a modern science. Though Aristotle watched the heart-beats of the unhatched chick, and had hold of the idea that development is a progressive differentiation and not an unfolding of preformed parts, he had practically no successors before Harvey (1578–1675).

William Harvey.—With the aid of magnifying

glasses (*perspecillæ*) Harvey demonstrated the connection between the "*cicatricula*" of the yolk and the rudiments of the chick, and he also observed some of the stages of uterine gestation in mammals. He maintained (1) that every animal is produced from an ovum (*ovum esse primordium commune omnibus animalibus*), and (2) that the organs arise by new formation (epigenesis) and not from the mere expansion of some invisible preformation, or, in other words, that in the primordium "no part of future organism exists *de facto*, but all parts inhere *in potentia*." But it has to be carefully remembered that he had no way of accounting for the primordium with which he started; he admitted that it might proceed from parents, or might arise spontaneously, or out of putrefaction. It was not he who coined the aphorism "*omne vivum ex ovo*," for which he often gets credit. Even if he had said it, the statement would not have meant to him what it means to us.

Early Observations.—Malpighi (1672), using a microscope with remarkable skill, traced back the chick-embryo into the recesses of the cicatricula lying on the top of the yolk, but he missed a magnificent discovery by supposing that the rudiments of the organs pre-existed in the egg. Spermatozoa were, it is generally believed, discovered by Leeuwenhoek's pupil, Ludwig Hamm, in 1677, though Hartsoeker afterwards claimed priority by three years:—a question of little interest, since neither understood what he saw. In 1664, Steno had given the ovary its present designation, and De Graaf had interpreted the vesicles of this organ ("the Graafian follicles") as for the most part equivalent to the ova which he thought he had discovered in the oviduct.

Theory of Preformation.—In spite of the begin-

nings of embryological observation in the seventeenth century, there was little progress for another hundred years. For the eighteenth century embryologists, if so they may be called, gave more attention to arguments over general conceptions than to the accumulation of facts.

In the early part of the eighteenth century, the embryological observations of investigators, like Boerhaave and Malpighi, were summed up in the conception that development was merely an expansion or unfolding of a pre-existent or preformed rudiment within the egg.

This preformation theory, which found more and more definite expression in the works of Bonnet, Buffon, and others, may be thus summed up:—*

The germ, whether egg-cell or seed, was believed to be a miniature model of the adult. "Preformed" in all transparency the organism lay within the egg, only requiring to be unfolded. In contrast to Harvey's conclusion: "the first concrement of the future body grows, gradually divides, and is distinguished into parts; not all at once, but some produced after the others, each emerging in its order," was Haller's first and last utterance, "There is no becoming; no part of the body is made from another, all are created at once," or Bonnet's "fundamental principle, that nothing is generated, and that what we call generation is but the simple development of what pre-existed under an invisible form, and more or less different from that which becomes manifest to our senses."

But this was not all. The germ was more than a

* See Geddes and Thomson, *The Evolution of Sex*, 4th ed., 1901, p. 90.

marvellous bud-like miniature of the adult, it necessarily included in its turn the next generation, and this the next—in short all future generations. Germ within germ, in ever smaller miniature, after the fashion of an infinite juggler's box, was the corollary of "*emboîtement*,"—logically appended to this theory of preformation and unfolding,—of *evolution*, as it was then called, in a very different but more literal sense from that in which we now use the word.

"The whole chapter is a somewhat lamentable one in the history of embryology, and yet it must be noted in fairness that the preformationist doctrine had a well-concealed kernel of truth within its thick husk of error. There is a certain sense in which the whole future organism is potentially and materially implicit in the fertilised egg-cell; there is a sense in which the germ contains not only the rudiment of the adult organism, but of successive generations as well. But in neither of these senses was preformationism understood by any of its upholders." *

In 1759 Caspar Friedrich Wolff (1733-1794) raised a strong protest against the doctrines and methods of the preformationists. He showed that the egg does not contain a preformed embryo, but that the organs were *to be seen* being formed. But his vindication of "epigenesis" against "evolution" did not win conviction as it ought to have done; indeed it remained for about sixty years without effect.

In 1817 Christian Pander took up embryological research where Wolff had left it, and worked out the

* See the writer's *Science of Life*, 1899, p. 121.

history of the chick in more exact detail. In 1824, Prévost and Dumas observed the division of the frog's ovum into masses. In 1827, Von Baer fulfilled, after a century and a half, what De Graaf had attempted, he discovered the mammalian ovum and traced it from uterus to oviduct, and thence to its position in the ovary itself. Soon afterwards, Wagner, Von Siebold, and others elucidated what was still hidden from Von Baer—the real nature of the spermatozoon. Kölliker began to trace the cells into which the ovum divides to their results in the tissues of the developing organism. In short, embryology began to get a firm basis.

Von Baer.—The foundation of modern embryology may be dated from the work of Karl Ernst von Baer (1792–1876). He broadened embryology as Cuvier has broadened anatomy, as Johannes Müller afterwards broadened physiology,—by making it comparative. He showed how the development of an embryo proceeded from the general to the special. He was the first to show, though his own illustrations have not survived, how embryological facts may be of service in classification.

Von Baer is linked to Francis Balfour by many illustrious workers in embryology:—Alex. Agassiz, Claus, Gegenbaur, Goethe, Haeckel, His, Kölliker, Kowalevsky, Leuckart, Loven, Metschnikoff, Johannes Müller, Ratke, Remak, Sars, Semper, Van Beneden, and many others. A strong stimulus was given by Balfour's monumental text-book (1880–1881), and in the last twenty years embryology has been the most progressive department of biology.

The Germ-Cells.—The cell-theory (1838–39) enunciated the important fact that every multicellular organism, if reproduced in the ordinary way, be-

gins its life as a cell; in short, that the egg is a cell. Somewhat later (1841) Kölliker traced the spermatozoa to their origin in the essential male organs or testes, and it was soon recognised that the spermatozoon also is a cell. We now know that both ovum and spermatozoon may show a complexity of minute organisation which was not suspected in the first half of the century, but this after all is a matter of detail.

The fundamentally important fact, which differentiates modern embryological conceptions from those of the first half of the nineteenth century is the idea of genetic continuity. This may be especially traced to the work of Virchow (1858), though several others were approaching it about the same time.

"To the modern student the germ is, in Huxley's words, simply a detached living portion of the substance of a pre-existing living body carrying with it a definite structural organisation characteristic of the species." * In other words, an egg or a sperm liberated from or set apart in any organism is connected by a lineage of cell-divisions with the fertilised ovum which gave rise to that organism, and so on backwards. It was an epoch-making step when embryologists arrived at "the conception so vividly set forth by Virchow of an uninterrupted series of cell-divisions extending backward from existing plants and animals to that remote and unknown period when vital organisation assumed its present form. Life is a continuous stream. The death of the individual involves no breach of continuity in the series of cell-divisions by which the life of the race flows onwards. The individual body dies, it is true, but the germ-cells live on, carrying with them, as

* E. B. Wilson, *The Cell in Development and Inheritance*, 2nd ed., 1900, p. 7.

it were, the traditions of the race from which they have sprung, and handing them on to their descendants." *

Fertilisation.—In his 49th Exercitation on "the efficient cause of the chicken," Harvey thus quaintly expressed what was to him, as it is to us, a baffling problem:—"Although it be a known thing subscribed by all, that the fœtus assumes its original and birth from the male and female, and consequently that the egge is produced by the cock and henne, and the chicken out of the egge, yet neither the schools of physicians nor Aristotle's discerning brain have disclosed the manner how the cock and its seed doth mint and coine the chicken out of the egge."

Even after Spallanzani had shown experimentally (1786) that the fertilising power must be in the minute spermatozoa, since filtered spermatic fluid of frogs was inoperative, vague and even absurd views continued to abound.

"Even von Baer (1835) was inclined to interpret the spermatozoa as minute parasites peculiar to the male fluid; Johannes Müller seems also to have been in doubt; and Richard Owen included them in his article on 'Entozoa' (internal parasites) in Todd's *Cyclopædia of Anatomy and Physiology*." † In 1843 Martin Barry saw the union of sperm and ovum in the rabbit, but it was not till 1854 that Bischoff abandoned the theory that a mere touch of sperm and ovum was sufficient to ensure fertilisation.

In fact, the distinctively modern period in the study of fertilisation only began about a quarter of a century ago, when the researches of Auerbach, E. van Beneden, Bütschli, Fol, De Bary, Strasburger,

* E. B. Wilson, *op. cit.*, p. 10.
† Thomson, *Science of Life*, p. 125.

Oscar Hertwig, and others made it clear that fertilisation in plants and animals alike is an intimate and orderly union of a spermatozoon and an ovum, —a union in which the two nuclei play a very important part.

It is generally believed that the paternal and maternal hereditary qualities which are united in fertilisation have their seat in the sperm-nucleus and the ovum-nucleus, especially or exclusively in the readily stainable or chromatin substance of these; as the ovum is very much larger than the spermatozoon, it evidently supplies most of the initial capital of cell-substance; the spermatozoon, however, contributes, apart from its nucleus, a little body called the centrosome which is now well known in many cases of animal fertilisation, and seems to play an important part in the process of egg-cleavage; the result of the cleavage is that each daughter-cell gets an equal share of the heritage of chromatin.

We have alluded to the importance of the idea of genetic continuity—that the germ-cell is a link in a continuous chain of germ-cells; but we must place close beside it the striking fact, which is for some stages visibly demonstrable, that the maternal and paternal chromatin-contributions which come together in fertilisation are distributed equally in the cells of the offspring.

During the last quarter of the nineteenth century there were many hundreds of researches on fertilisation, and there is perhaps a larger amount of observational material on this subject than on any other except cell-division, but it must not be supposed for a moment that the process is understood. The general tendency, following Hertwig and Strasburger, is to credit the nuclei with being alone im-

portant in the process, but against this we have the facts—as yet uncontroverted—that a non-nucleated ovum or even fragment of an ovum may be fertilised and may develop to the larval stage (Boveri and Delage), and that artificial conditions may induce an ovum to develop without a spermatozoon. Thus, Loeb induced artificial parthenogenesis in sea-urchin ova by placing them for a couple of hours in sea-water, to which some magnesium chloride had been added, disturbing the normal proportions of the ions. There are also incipient experiments (Pieri, Winkler, and others) on the effect of an extract of sperm in stimulating the cleavage of the ovum. Everything points to the desirability of extreme caution, but it seems likely that we have to distinguish in fertilisation two distinct results—(*a*) a mingling of heritable qualities, and (*b*) a physiological stimulus to division.*

Since the formulation of the Cell-Theory, the development of Embryology has been rapid, and this may in part account for the insecurity of its generalisations. We propose to refer to a few of these.

Germ-Layers.—The fertilised animal ovum divides into a mass of cells—a solid ball, or morula; a hollow ball, or blastula; a convex disc on the top of the yolk, and so on. The next great step is the differentiation of two germinal layers—the diploblastic state. Of these the outer layer is called the ectoderm or epiblast, and the inner the endoderm or hypoblast. When the egg is not encumbered with much yolk this two-layered stage most frequently assumes the form of a thimble-shaped or barrel-shaped embryo, whose cavity is the primitive gut or

* See Geddes and Thomson, *The Evolution of Sex*, revised (4th) edition, 1901.

archenteron. The ectoderm gives rise to epidermis, nervous system, foundations of the sense-organs and so on; the endoderm forms the lining of the future mid-gut and of the various organs (such as lungs, liver, and pancreas) which grow out as diverticula from it, and likewise, in vertebrates, to the primitive dorsal axis or notochord; while a third median stratum of cells—the mesoderm—of considerable definiteness above the level of the unsegmented worms, gives origin chiefly to muscular and skeletal tissue.

From the work of Von Baer onwards much attention has been paid to these germinal layers; in 1849 Huxley collated the epiblast and hypoblast of the embryo with the two layers of cells which form the body of adult polyps, like the common Hydra; and it was regarded as one of the criteria of complete homology that organs similar in structure should also be homodermic, i.e., traceable to a similar origin from the germinal layers. The work of the brothers Hertwig in connection with this germ-layer-theory (*Keimblättertheorie*) was of particular importance.

"Gradually, however, the confidence of embryologists in this germ-layer-theory has been shaken—by the following, among other, considerations. (*a*) What one may call the stratification of the embryo is established in very different ways in different types; (*b*) there are some cases, notably sponges, where the history of the outer and inner layers cannot be readily brought into line with the state of affairs in the majority; (*c*) the mesoderm is so varied in its origin (from ectoderm, from endoderm, or from both) and in its expression, that the conception lacks even a pretence at unity; and (*d*) in many cases the facts of development show that certain

organs can be traced back to a few cells specifically predestined from their first appearance, rather than to a homogeneous germinal layer." * In fact, the germ-layer-theory is now regarded by many experts as "inadequate and misleading," and it is being replaced by a more detailed study of cell-lineage in which segmentation-cells or blastomeres are traced from their origin to their final result.

Gastræa-Theory.—The same kind of remark must be made in regard to Haeckel's famous *Gastræa-Theory* (1874). In this there are two propositions, —(1) that the gastrula-embryo (the two-layered sac) is of general occurrence, though often disguised, in the development of animals; and (2) that the hypothetical ancestral form of multicellular animal (the Gastræa) was a two-layered sac like a gastrula. But it requires extraordinary ingenuity to find the gastrula-stage in, let us say, the development of a hedgehog, or even in that of the chick. And even when the gastrula is plain, as in starfishes, it is not always clear that its layers are homologous with those of other gastrulæ, e.g., in Sponges. As to the other part of the Gastræa-Theory, there are three or four plausible hypotheses in the field as to the possible form of the ancestral multicellular animal. It is likely enough that there were several forms.

Recapitulation-Doctrine.—Once more, to take the largest generalisation of nineteenth-century embryology,—the Recapitulation-Doctrine or biogenetic law,—which suggests that the individual development is in some measure a recapitulation of the racial history, there are few modern embryologists who regard it without hesitation and suspicion.

* *Science of Life*, p. 131.

Meckel in 1821 was one of the first to speak of the "correspondence between the development of the embryo and that of the entire animal series." Kielmeyer seems to have something to do with the origination of the idea; Oken and Goethe both express it. Von Baer, to whom the recapitulation-idea is often carelessly ascribed, was very cautious on the subject; Louis Agassiz (though a non-evolutionist) gave it clear expression in his famous *Essay on Classification* (1859); his son Alexander was also an adherent, though more guardedly; Fritz Müller was an enthusiastic exponent in his *Facts for Darwin*, Haeckel formulated it in his "*Biogenetisches Grundgesetz*" (fundamental biogenetic law) that "Ontogeny tends to recapitulate Phylogeny"; and Herbert Spencer also made it part of his biological system.*

There is no doubt that we have here a big idea and a clear one, that of individual development in some measure recapitulating racial history, and it must not be hastily condemned because of popular exaggerations on the one hand (no idea has suffered more from its friends), or because critics have sought rather to controvert than to correct it. Let us admit the grotesqueness of popular exposition, e.g., that the mammal is at one time a little fish; let us allow that Milnes Marshall did not mean to be taken too literally when he spoke of "every animal climbing up its own genealogical tree"; let us grant that evidence from the child's acquirement of language and ideas is not very cogent evidence of parallelism to a past which is more than half-concealed; let us remember Haeckel's explicit declaration that the

* For some details, see the writer's *Science of Life*, pp. 133-136.

recapitulation is general, not exact, that there is often a tendency to abbreviation, and that relatively recent adaptations (kainogenetic characters) may disguise the ancient ancestral features (palingenetic characters); let us emphasise that the recapitulation-idea was not intended as a contribution to the physiology of development, but was merely suggested as a historical interpretation—a light from a distance; and let us even acknowledge that more exact knowledge sees differences where more hasty earlier observations saw only resemblances. Yet, after all, there is a good word to be said for the recapitulation idea.

If we take an individual animal, like the frog, and study its life-history, we cannot but conclude that *in a general way and in respect to certain changes in organs,* its ontogeny does recapitulate its phylogeny.

But let us notice two possible fallacies. In summing up the so-called, we think miscalled, "evidences of evolution," it is customary to cite a case like that of the frog's life-history—with its fish-like and dipnoan-like stages—as part of the "evidence." The frog, in its tadpole and other stages, is supposed to oblige the naturalist—the evolutionist—by climbing up its own genealogical tree; and that it does so is cited as a corroboration of the evolution-idea. But when we come to study the frog's development in itself, as part of the practical course of embryology, and are puzzled by its circuitousness, we explain (or are tempted to explain) the turns and twists of the ontogeny by saying, that in so doing the larval frog is recapitulating the historical evolution of its race.

The second fallacy is this, that when we examine

the facts carefully it is at once evident that the larval frog (or tadpole in the wide sense) is *never* a little fish, though it has undoubtedly a fish-like heart, a fish-like circulation, and fish-like gills. It is, none the less, from the very outset an amphibian, and even more than that a frog; whether we consider its scaleless skin with multicellular skin-glands, or its muscular tongue, or its rayless dorsal fin, or its posterior nares, or a dozen other features, it is an amphibian from beginning to end. The parallelism is rather between the development and the phylogeny of *organs*, than between the life-history and the evolution of *organisms*. And even in regard to organs, the recapitulation-doctrine in its cruder forms breaks down, for in Rabl's recent monograph on the lenses of vertebrates, it is clearly shown that although in the development of the higher lenses (of mammals, for instance) there is some recapitulation of the evolutionary stages, yet the earliest rudiment of the lens (of a cat or of a bat) is specifically peculiar in every case.

Probably as the result of rapid development, the generalisations of embryology—such as the germ-layer-theory, the gastræa-theory, the recapitulation doctrine,—are no longer tenable without many saving-clauses. But, since each, undoubtedly, expresses some truth, our endeavour should be not that of destructive criticism, but rather that of adapting them to the new data.

Physiological Embryology.—What Pander and Lotze suggested,—that there should be an enquiry into the immediate conditions which are operative in development, was recognised by His in the famous work *Unsere Körperform und das Problem ihrer Entstehung* (1875), and by Rauber in his *Formbil-*

dung und Formstörung (1880). "To think that heredity will build up organic beings without mechanical means" is, according to His, "a piece of unscientific mysticism"; and from many different sides there has been an attempt to analyse the processes of organic growth and embryonic architecture. The task, which is involved in stupendous difficulties, has been touched by the work of O. Hertwig, Pflüger, Fol, Born, O. Schultze, Berthold, Gerlach, Van Beneden, Boveri, Heidenhain, Loeb, Davenport, and many others, but the name of Roux should be particularly associated with the attempt to get nearer some concrete conception of developmental mechanism.

"Developmental mechanics," he says, "or the causal morphology of organisms, is the doctrine of the causes of organic forms—the doctrine of the causes of the origin, maintenance, and involution (degeneration) of these forms. . . . In any given case, we must trace back each individual formative process to the special combination of energies by which it is conditioned, or, in other words, to its *modi operandi,* and each of these *modi operandi* must be ascertained with respect to place, time, direction, magnitude, and quality. Or, inversely we may endeavour to determine in the individual structure the special part which is performed by every *modus operandi* known to participate in the development of the organism."

To mention those who have helped Roux towards the realisation of this ambitious aim would be to give a list of the contributors to the *Archiv für Entwickelungs-Mechanik.* But this could serve no useful purpose.

The problem of development has been passed on to the twentieth century quite unsolved, and we can-

not here discuss the various theories. It may be said, however, that each step in development is a function of three factors: (*a*) the organisation of the germ-cells, objectively expressed in a visible complexity of structure, and in an inconceivable molecular complexity beneath this: (*b*) the vital relation of the various blastomeres or segmentation-cells to one another; and (*c*) the environmental influences (pressure, osmosis, chemical composition of the medium, temperature, light, etc.) which play upon the whole.

EXPERIMENTAL EMBRYOLOGY.

Although the idea of artificially influencing the germ is very old, although even Swammerdam is said to have succeeded in producing monstrosities, experimental embryology is practically a new departure in biology. Almost all the experiments of moment have been made in the last twenty years, and since 1890 it has been a prominent line of research. There is a Journal—*Archiv für Entwickelungs-Mechanik,* edited by Roux—which is in great part devoted to the subject, and there are already at least two text-books mainly devoted to its exposition.*

(*a*) One of the first modes of experiment in this direction was in the artificial production of monstrosities. Just as pathology sheds light on physiology—in the case of the thyroid gland for instance—so teratology and teratogenesis (the study and production of monstrosities) may help us to understand normal development. The most successful worker along this line has been Camille Dareste,† the

* W. Haacke, *Entwickelungs-Mechanik.* C. Labbé, *Cytologie Expérimentale.*

† *Recherches sur la production artificielle des monstruosités; ou Essais de Tératogénie Expérimentale,* Paris, 1877; 2nd ed., 1891.

acknowledged chief of monster-makers. He has experimented for instance, with the egg of the fowl,—a *corpus vile* for many purposes—placing it vertically instead of horizontally, keeping it slightly above or slightly below the normal temperature of incubation, heating different parts of the egg unequally, hermetically varnishing part of the shell, and so on. He has not only shown that the germ is plastic in the grip of its environment, but he has been able to induce a number of particular malformations which are of interest to the student of normal structure.

Of great importance, perhaps inadequately recognised, is the work of Prof. A. Rauber, *Formbildung und Formstörung* * (1880), which showed the significance of relating the results of abnormal disturbance to the normal sequence of events, and described a number of interesting experiments. To it we may refer the serious student for a historical sketch of the results achieved before 1880.

There are many other workers, such as O. Hertwig, B. C. A. Windle, and Ch. Féré, whose investigations are in part on the same lines as those of Dareste and Rauber.

(*b*) *Puncturing Experiments.*—The egg of the frog, about one-tenth of an inch in diameter, is a very convenient subject for embryological experiment. The first three cleavages, visible even with the naked eye, lie along three planes, which, in order of sequence, correspond to those which divide the tadpole into right and left sides, head and tail regions, dorsal and ventral areas. Of the first two cells into which the egg of a frog divides one has in it the material for forming the right half of the body, the other has

* *I.e.*, Forming and deforming.

in it the material for forming the left half of the body.

When Roux punctured one of the first two segmentation-cells (or blastomeres) with a hot needle or otherwise, he found that the intact other cell developed into a typical *half*-morula, or *half*-gastrula, or *half*-embryo, according to the success in survival. Thus, there might be in the embryo, half of the normal cerebrum, one ear-sac, a one-sided gut, a single row of protovertebræ, and so on. Thus it was proved that one of the first two segmentation cells (or blastomeres) may form half an embryo; it has the requisite material and the requisite power of independent development. This, and many similar experiments, led Roux to his theory that the early development of the frog-ovum is like a kind of mosaic work pieced together in independent parts. He suggested that there were at least four independently-developing pieces. It should be noted, however, that the half-embryo may eventually form a whole, either with the aid of a re-vitalisation of the injured half-egg which has been lying passive while the uninjured half was developing, *or* even without any co-operation on the part of the injured half of the first cleavage.*

So far, there seemed to be a definite conclusion reached by an investigator of the first rank, that the puncturing of one of the first two cells into which a frog's egg divides, has for its result that the intact other cell forms a half-embryo,—a one-sided embryo, which by "post-regeneration" may become eventually a whole.

But in 1893, Professor Oscar Hertwig, whose con-

* Virchow's *Archiv f. Pathologie*, CXIV. (1888).

tributions to biology have been momentous, published the results of an extensive series of experiments * on the same subject, and these were far from harmonising with the conclusion reached by Roux.

According to Hertwig, if one of the first two segmentation-cells (or blastomeres) be completely destroyed, the surviving half forms a fairly normal embryo, with structural defects of slight importance. If the destruction be partial, division may occur in the injured half, either in its own strength or with help from the intact half. But a destroyed half cannot be revitalised, nor does Roux's post-generation occur. The development of the uninjured half is quite normal. No half-gastrula or half-embryo is ever formed, when one of the first two blastomeres is destroyed. Therefore, as Hertwig concluded, the mosaic theory of development is contradicted by fact.

We wish to dwell upon this particular case because it is so vividly illustrative of scientific method. Here we have observers of equal competence reaching discrepant conclusions from similar experiments on the same material!

The puzzle was solved (in great part at least) by the very careful research of Prof. T. H. Morgan,† who showed that either a half-embryo or a whole half-sized dwarf may result from the experiment, *according to the position of the blastomere.* If, after one of the first two cells has been destroyed, the other be left in its normal position, then a half-embryo results (11 cases) as Roux described. But if the

* *Archiv für mikroskopische Anatomie,* XLII. (1893), pp. 662–807, 6 plates (with bibliography of 52 papers).

† Half-embryos or whole-embryos from one of the first two blastomeres of the frog's egg. Anat Anzeig., 1895.

intact blastomere be inverted, then it may develop into a half-embryo (3 cases) or into an entire dwarf (9 cases).

"Morgan therefore concluded that the production of whole embryos by the inverted blastomeres was, in part at least, due to a rearrangement or rotation of the egg-materials under the influence of gravity, the blastomeres thus returning as it were, to a state of equilibrium like that of an entire ovum." *

(*c*) *Isolation-Experiments.*—Professor C. Chun observed in 1877 that when the two first segmentation-cells of a ctenophore ovum were shaken apart, each formed a half-larva, with four instead of eight ciliated ridges and meridional vessels, with one tentacle instead of two. The half-larvæ actually became sexual, and by a process of budding, the missing half was eventually formed. The observer also added the interesting note that united twin ctenophore-larvæ were most abundant after stormy days, probably resulting from the incomplete separation of the first two blastomeres and their independent development.

The importance of Chun's hint was recognised by Driesch who was the first to develop the method of isolating segmentation-cells by shaking. The device has been resorted to in many cases,—with ascidians and sea-urchins in particular. As a particularly fine piece of work, we may refer to Prof. E. B. Wilson's experiments on the eggs of the lancelet (*Amphioxus*).†

By shaking the water in which the two-celled stages floated, Wilson separated the two cells, and

* E. B. Wilson, *The Cell in Development and Inheritance*, 2nd ed., 1900, p. 422.

† *Journal of Morphology*, VIII. (1893), pp. 579–638, 10 pls.

the result was two quite separate and independent twins of half the normal size. Each of the isolated cells segments *like a normal ovum,* and gives origin, through blastula and gastrula stages, to a half-sized metameric larva.

If the shaking has separated the two first segmentation cells incompletely, double embryos—like Siamese twins—result, and also form short-lived (twenty-four hours) segmented larvæ.

Similar experiments with the four-celled stages succeeded, though development never continued long after the first appearance of metamerism. Complete isolation of the four cells resulted in four dwarf blastulæ, gastrulæ, and even larvæ. Separation into two parts of cells resulted in two half-sized embryos. Incomplete separation resulted in one of three types—(*a*) double embryos, (*b*) triple embryos—one twice the size of the other two—and (*c*) quadruple embryos, each a quarter size.

The eager observer proceeded to shake up the eight-celled stages, but in no case did he succeed in rearing a gastrula from an isolated unit of the eight-celled stages. Flat plates, curved plates, even one-eighth size blastulæ were formed, but none seemed capable of full development.

Thus, a unit from the four cell stage may form an embryo, but a unit from the eight cell stage does not. For various reasons it seems likely that this is due to qualitative limitations, not merely to the fact that the units of the eight cell stage are smaller. For although the separated cells of the eight cell stage have considerable vitality, and swim about actively, the difference between macromeres and micromeres has by this time been established; in fact the cells have begun to be specialised, and have no longer the

primitive indifference, the absence of differentiation, which explains the developmental potentiality of the separated units of the two-celled or four-celled stages.

Somewhat similar experiments have been made by other investigators on the developing ova of ascidians, sea-urchins, etc. Specialisation of segmentation-cells appears to occur at different times in different animals, but it is illogical to infer the absence of specialisation from the fact that any of the first four blastomeres, let us say, can produce an entire embryo. For specialised cells may retain a power of regeneration.

(*d*) *Pressure-Experiments.*—Many investigators, e.g., Driesch, O. Hertwig, Born—have studied the behaviour of an ovum subjected to the constraint of slight pressure between glass plates. Prof. Hertwig shows that various compressions profoundly modify the course of segmentation, the direction and succession of the cleavage planes, and the size of the blastomeres. The nuclei may be most variably disposed, they may lie in disorder, "like a heap of balls thrown together," and *yet normal embryos result.* This is regarded by many as a strong argument against the theory that qualitatively different portions of the nucleus are separated from one another by the early cleavages.

Here we may also refer to the interesting results of rotating the eggs so that the distribution of their substance is affected by "centrifugal force." This may also have a profound effect on the segmentation; thus O. Hertwig has shown in the case of the frog's egg that the normal segmentation (total and unequal) may be replaced by a process closely akin to the type known as partial and discoidal.

(*e*) *Influence of Temperature.*—In his account of the development of one of the earthworms (*Allolobophora trapezoides*), whose eggs very frequently form twins, Vejdovsky suggested that the "twinning" was perhaps influenced by warmth, for it was most frequent in warm weather. This suggestion prompted Driesch to try the effect of increased warmth on the developing eggs of the sea-urchin (*Sphærechinus*). The usual result was very striking, though it was not quite constant, nor verifiable in related forms, e.g., *Echinus*. What often happened in the case of *Sphærechinus* was the formation of distinct twin-embryos and even twin-larvæ (Plutei) from each egg.

In a later series of experiments, Driesch showed that when the blastula-embryos (hollow balls of cells) of *Sphærechinus granularis* are kept in sea-water on a stove heated to about 30°C., the great majority show in about 18 hours a remarkable state of affairs (exogastrula-state) in which the area of cells which is normally invaginated to form the primitive gut, bulges outwards instead of inwards. The final result, which may survive for a week, is a gut-less larva—an "Anenteria."

Many other experiments, both as to heat and cold, have been made, and they are probably of great importance since vicissitudes of temperature are of frequent occurrence in natural conditions. It may be conjectured that the temperature influences the metabolism of the cells, e.g., the rate of formation of nuclein-compounds, and thus affects the manner of growth.

(*f*) *Influence of Chemical Re-agents.*—In 1887, O. and R. Hertwig published a pioneer-research on the influence of chemical and other stimuli on fertil-

isation and cleavage.* This was the beginning of a long series of researches, of which the most remarkable are probably those of Curt Herbst.†

Herbst placed fertilised ova of various sea-urchins in sea-water whose normal composition had been disturbed by the addition of solutions of potassium chloride, lithium chloride, and so on, usually in the proportions of 3.8 grms. to 100 centimetres of sea-water. Nothing could be quainter than some of the resulting abnormal forms which nevertheless tended to reach the normal (Pluteus) type by entirely abnormal paths. It remains uncertain how far the chemical re-agents act directly, or only by disturbing the osmotic pressure, but Herbst favours the second interpretation.

(*g*) *Loeb's Experiments.*—Profs. O. and R. Hertwig, Prof. T. H. Morgan, and others have shown that if unfertilised eggs (especially of sea-urchins) be subjected to the influence of weak solutions of various salts (sodium-chloride, magnesium-chloride, etc.) or of other substances (such as strychnine), they may exhibit changes comparable to those of cleavage or of preparation for cleavage.

In 1899, Professor Jacques Loeb of Chicago succeeded in rearing perfect larvæ of sea-urchins from unfertilised eggs which had been left for a couple of hours in sea-water disturbed by the addition of some magnesium chloride. It seems to us impossible to find any reason for doubting the accuracy of the carefully controlled experiments.‡ It may be, however, that sea-urchin ova are sometimes

* *Jenaische Zeitschrift f. Naturwissenschaften*, XX., 1887.

† *Zeitschr. wiss. Zool.*, LV., 1892, pp. 446-518, 2 pls. *Mt. Zool. Stat. Neapel*, XI. 1893, *Archiv f. Entwickelungs-Mechanik*, II., 1896, etc.

‡ *American Journal of Physiology*, 1899 and 1900.

parthenogenetic in natural conditions, but this is only a supposition and will not, even if verified, affect the interest of Loeb's experiments.

(*h*) *Boveri's Experiment.*—The brothers Hertwig showed that non-nucleated fragments of a sea-urchin's egg might be "fertilised" by a spermatozoon, and might segment. In 1889, Boveri proved that they might form dwarf larvæ, and Morgan in 1895 demonstrated that the nuclei of such larvæ contained only half the normal number of nuclear elements or chromosomes,—an indication of the fact that the nuclear material was wholly paternal, i.e., derived from the sperm-nucleus.

"Now, by fertilising enucleated egg-fragments of one species (*Sphærechinus granularis*) with the spermatozoa of another (*Echinus tuberculatus*), Boveri obtained in a few instances dwarf Plutei (larvæ) showing except in size *the pure paternal characteristics,* i. e., those of the *Echinus*. From this he concluded that the maternal cytoplasm has no determining effect on the offspring, but supplies only the material in which the sperm-nucleus operates. Inheritance is, therefore, effected by the nucleus alone.

"The later studies of Seeliger (1894), Morgan (1895), and Driesch (1898) showed that this result is not entirely conclusive, since hybrid larvæ arising by the fertilisation of an entire ovum of one species by a spermatozoon of the other show a very considerable range of variation; and while most such hybrids are intermediate in character between the two species, some individuals may nearly approximate to the characters of the father or the mother. Despite this fact, Boveri (1895) has strongly defended his conclusion, though admitting that only further research can definitely decide the question." *

* E. B. Wilson, *The Cell in Development and Inheritance*, 2nd ed., 1900, p. 353.

(*i*) *Delage's Experiments.*—In a short paper entitled "Embryos without Maternal Nucleus," Professor Yves Delage described in 1898 * a remarkable experiment, implying a very delicate operation. He divided the egg of a sea-urchin under the microscope into two parts, one containing the nucleus and the centrosome, the other simply cytoplasmic. Beside them he placed an intact ovum, and then let spermatozoa in. All the three objects showed equal "sexual attraction," all were "fertilised," and all segmented, the intact ovum most rapidly, the nucleated fragment more slowly, the non-nucleated fragment more slowly still. In one case, the development proceeded for three days; the intact ovum had become a typical gastrula (two-layered embryo), the nucleated fragment a smaller gastrula, and the non-nucleated fragment also a gastrula, but with a very much reduced cavity. The experimenter therefore concluded that fertilisation and some measure of development may occur in a fragment of ovum without a maternal nucleus; and he was led to distinguish between (*a*) the stimulus given to the ovum by something which the spermatozoon brought to it, and (*b*) the mingling of heritable characteristics—as two distinct processes in fertilisation.

In the following year, Delage extended his experiments,† and showed that a non-nucleated fragment of the ovum of a sea-urchin (*Echinus*), of a mollusc (*Dentalium*), and of a worm (*Lanice*) may be effectively fertilised and give rise to a Pluteus, a Veliger, or a Trochophore larva respectively. He

* *Comptes Rendus Acad. Sci., Paris*, CXXVII., 1898. pp. 528-531.

† *Archives Zoologie Expérimentale*, VII. (1899), pp. 383-417, 11 figs.

showed that three larvæ may be reared from a single sea-urchin ovum divided into three pieces, and that a normal blastula might develop from $\frac{1}{37}$ of an ovum. To this development of fragments he applied the term merogony.

It will be observed that while Loeb showed that normal development was possible without the paternal nucleus, Delage showed that this was possible without the maternal nucleus. If both sets of experiments are duly confirmed, there will be need for some reconstruction in the current views as to fertilisation.

(*j*) *Determination of Sex.*—A reference should be made here to the numerous experiments on the factors which determine whether a germ is to become a male or a female organism. The investigations of Born, Pflüger, Yung, Maupas, Nussbaum, and Düsing are of especial importance; but we may refer for detailed discussion to *The Evolution of Sex* (4th edition, revision) by Prof. Patrick Geddes and the writer, and to the dispassionate review by Henneberg (*Anatomische Ergebnisse,* Merkel and Bonnet, VII., 1897; pp. 697–721). We must be content with a general summary:—

The epoch at which the sex is finally determined is variable in different animals, and diverse factors operative at successive epochs. Theological and metaphysical theories of sex have preceded the scientific; observation and statistics have been resorted to before experiment; and over 500 theories in all have been set forth. That there are two kinds of ova is still for the most part an assumption; that the entrance of more than one spermatozoon frequently occurs, and is a determining factor, is erroneous. Thury's emphasis on the age of the ovum

when fertilised is probably justified; while Hensen extends this notion to the male element as well. The age of the parents is probably only of secondary import, and the law of Hofacker and Sadler as to the importance of this is not confirmed. Theories of "comparative vigour" and the like must be dismissed; while Starkweather's theory of the relative superiority of either sex, and of the influence of this on the sex of the offspring, requires further analysis. But there is much importance in Düsing's explanation of the self-regulating numerical proportion of the sexes.

It must first be recognised that a number of factors co-operate in the determination of sex; but the most important of these may be more and more resolved into plus or minus nutrition, operating upon parent, sex elements, embryo, and in some cases larvæ. (*a*) Starting with the parent organisms themselves, we find this general conclusion most probable,—that adverse circumstances, especially of nutrition, but also including age and the like, tend to the production of males, the reverse conditions favouring females. (*b*) As to the reproductive elements, a highly nourished ovum, compared with one less favourably conditioned, in every probability will tend to a female rather than to a male development. Fertilisation, when the ovum is fresh and vigorous, before waste has begun to set in, will corroborate the same tendency. (*c*) Then if we accept Sutton's opinion as to a transitory hermaphrodite period in most animals, from which the transition to unisexuality is effected by the hypertrophy of the female side or preponderance of the male in respective cases, the vast importance of early environmental influences must be allowed. The longer the period of

sexual indifference (though this term be an objectionable one) continues, the more important must be those outside factors, whether directly operative or indirectly through the parent. Here again, then, favourable conditions of nutrition, temperature, and the like, tend towards the production of females, the reverse increase the probability of male preponderance.

The general conclusion, then, more or less clearly grasped by numerous investigators, is that favourable nutritive conditions tend to produce females, and unfavourable conditions males.

"Let us express this, however, in more precise language. Such conditions as deficient or abnormal food, high temperature, deficient light, moisture, and the like, are such as tend to induce a preponderance of waste over repair—a *relatively katabolic* habit of body,—and these conditions tend to result in the production of *males*. Similarly, the opposed set of factors, such as abundant and rich nutrition, abundant light and moisture, favour constructive processes, i. e., make for a *relatively anabolic* habit, and these conditions tend to result in the production of *females*. With some element of uncertainty, we may also include the influence of the age and physiological prime of either sex, and of the period of fertilisation. But the general conclusion is tolerably secure,—that in the determination of sex, influences inducing a relative predominance of katabolism tend to result in production of males, as those favouring a relative predominance of anabolism similarly increase the probability of females." *

(*k*) *Other Experiments.*—(1) The importance of the age or staleness of the germ-cells in affecting the growth of the embryo has been carefully studied

* *Evolution of Sex*, 4th ed., 1901, p. 55.

by Dr. H. M. Vernon.* (2) Heape is responsible for a number of experiments on artificial insemination, and for such daring experiments as the following.† From an Angora doe rabbit (fertilized 32 hours previously by an Angora buck) he transferred two ova into the upper end of the oviduct of a Belgian doe rabbit (inseminated three hours previously by a Belgian buck), with the result that when the Belgian doe gave birth, four of the young were Belgian and two Angoras. (3) Prof. Cossar Ewart's "Penycuik Experiments" have added not a little to our knowledge as to the variable results of hybridisation and as to the occurrence of reversions.‡ (4) The experiments of Ritzema-Bos and others as to in-breeding (in rats) suggest that there are limits beyond which this is likely to prove very disadvantageous.

(5) Of the utmost importance, as indeed a beginning of an experimental study of the conditions of reproduction in plants, has been the careful work of Klebs (1896), in which he has shown how changes in the environmental conditions may induce, in Algæ and Fungi, the occurrence of sexual or asexual reproduction. The factors investigated were nutrition, moisture, light, temperature, and chemical reagents; and the general result is a proof that certain external conditions determine the occurrence of asexual reproduction (by zoospores), while others as certainly evoke sexual reproduction (by gametes).

(6) *Maupas' Experiments.*—Though the work of Maupas, like that of Klebs, has chiefly to do with

* *Proc. Roy. Soc. London*, LXV. (1899), pp. 350-360.
† *Proc. Roy. Soc., London*, XLVIII. (1891), pp. 457-58.
‡ The Penycuik Experiments, 1899.

unicellular organisms and not with embryos, this seems the fittest place to take note of both, and it must be remembered that the Protozoa and Protophyta stand to the whole race of animals and plants in somewhat the same relation as the germ-cells and embryos do to individual organisms.

As the result of a long series of observations—models of patient accuracy—Maupas reached the general conclusion that sexual union in ciliated Infusorians, dangerous perhaps for the individual life, —a loss of time so far as immediate multiplication is concerned,—is necessary for the continued vigour of the race. The life runs in cycles of asexual division, which are strictly limited. Conjugation with unrelated forms must occur, else the whole life ebbs. Without it, the Protozoa, which some have called "immortal," die a natural death. Conjugation is the necessary condition of their eternal youth and immortality. Even at this low level, only through the fire of love can the phœnix of the species renew its youth.*

(*l*) *Regeneration Experiments.*—In the eighteenth century, the attention of naturalists was for a time focussed on the problem of the regeneration or regrowth of lost parts. Trembley discovered to his great delight that the fresh-water polyp (*Hydra*) might be multiplied by being cut in pieces; Spallanzani showed that the earthworm cut by the spade might regrow a new tail or even a new head; Bonnet made numerous experiments on other worms, and thought out an elaborate theory; Réaumur pointed out the advantage of the regenerative capacity in animals which were in their natural conditions exposed to frequent risks of breakage or

* See *Evolution of Sex* (4th ed., 1901), pp. 176–78.

wounds. Neither facts nor interpretations were a-wanting a hundred years ago.

Towards the end of the nineteenth century the problem of regeneration came again to the forefront of biological enquiry. The basis of fact was broadened, and the interpretations became less vague.

The regenerative capacity is very unequally distributed in the animal kingdom; it is often exhibited in regard to external parts, but rarely in regard to internal parts. Its mechanism remains very obscure, but there seems much reason to accept the interpretation, which has occurred to many naturalists from Réaumur to Weismann, but was summed up in Lessona's law (1868)—that regeneration tends to be well-marked in those animals and in those parts of animals which are in the course of natural life very liable to injury. To this we may add two saving-clauses,—(*a*) always provided that the lost part is of some vital importance, and (*b*) that the wound or breakage is not fatal. The theory, the Darwinian interpretation as we may call it, is, in Weismann's words, that "the power of regeneration possessed by an animal or by a part of an animal is regulated by adaptation to the frequency of loss and to the extent of the damage caused by the loss." The importance of comparing regenerative processes with those of normal development is obvious, even though both remain unread riddles. The researches of Weismann and Morgan, Barfurth and Bordage, Werner and Wheeler, Wolff and Müller, Loeb and Michel, are of special importance.

In the last quarter of the nineteenth century embryology, hitherto observational, became more definitely experimental. Dareste and Rauber were pioneers on a line of research which has been fol-

lowed by many workers,—the Hertwigs, Roux, Driesch, Herbst, Wilson, Morgan, Loeb, Delage, and many others. The results have contributed (a) *to the morphological problem of cell-lineage,* (b) *to the physiological problem of growth-conditions or body-physics,* (c) *to the general theory of the meaning of fertilisation and development, and* (d) *to our knowledge of the influence of the environment in inducing modifications. But it is too soon to appreciate the results, some of which seem mere curiosities, while others suggest a revolutionary change of outlook.*

HEREDITY AND INHERITANCE.

Old Problems, but a Modern Study.—Even in ancient times men pondered over the resemblances and differences between children and their parents, and wondered as to the nature of the bond which links generation to generation. But although the problems are old, the precise study of them is altogether modern, and may almost be called Darwinian. For it was largely under Darwin's influence, dating from the publication of the *Origin of Species* (1859), that the scientific study of the problems of heredity began. The other chief influence was the cell-theory, especially that development of it which Virchow expounded—the idea of genetic continuity. It should also be remembered that the first adequate presentation of the facts of inheritance was published about the middle of the century, namely, *Traité de l'hérédité naturelle* (1847–1850), by Prosper Lucas, which furnished a useful basis for more critical enquiry.

Let us briefly notice some of the changes since the beginning of Darwin's day.

(1) Before the middle of the nineteenth century much attention was given to what might be called the demonstration of the general fact of inheritance. Hundreds of pages in the treatise of Prosper Lucas are devoted to proving that the present is the child of the past, that our start in life is no haphazard affair, but is rigorously determined by our parents and grandparents, and that all sorts of innate peculiarities—both great and small—may reappear generation after generation. Nowadays, no one doubts the general fact; almost everyone rather will agree with Prof. E. B. Wilson that "the studies of Darwin, Galton, and others have shown that there is no peculiarity of structure or function in any part of the body too slight to escape the influence of either parent or both in inheritance. . . . Both parents affect the whole development of the child and may exert an influence on every detail of its organisation." *

It is hardly too much to say that in the development of natural knowledge, science begins where measurement begins. And this is the case in regard to inheritance. Or, perhaps, instead of measurement, which may be taken in too narrow a sense, we should say that precision of observation and record which admits of statistical, mathematical, or some other exact formulation. While nothing can take the place of experiment—which is urgently needed for the further development of our knowledge of heredity—much has been gained by the application of statistical and mathematical methods to biological results—a new contact between different disciplines

* International Monthly, II., July, 1900, p. 80.

—which we may particularly associate with the names of Mr. Francis Galton and Mr. Karl Pearson.

(2) A second step is the further elucidation and widespread acceptance of the idea which Virchow was one of the first to state,—the somewhat subtle, yet essentially simple idea, which may be called "*the continuity of generations.*"

There is a sense, as Mr. Galton says, in which the child is as old as the parent, for when the parent's body is developing from the fertilised ovum, a residue of unaltered germinal material is kept apart to form the reproductive cells, one of which may become the starting-point of a child. Similarly, Weismann, generalising from cases where it seems to be visibly demonstrable, maintains that the germinal material (*germ-plasm*) which starts an offspring owes its virtue to being materially continuous with the germinal material from which the parent or parents arose.

(3) A third step is that we are learning not to spell heredity with a capital. We no longer think of it as a power or principle, as a fate or as one of the forces of nature; we study it as a relation of genetic continuity between successive generations, in a sense mysterious, as every fact of life is, but none the less a relation sustained by a visible material basis (the germ-cells) and expressing itself in resemblances and differences which can be measured and weighed.

The very terms "heredity," "heritage," "inheritance," "transmit," are perhaps apt to deceive us by their suggestion of a false analogy. In regard to property there is a clear distinction between the heir and the estate which he inherits; in regard to life

there is at first no such distinction. We inherit *ourselves;* organism and inheritance are, to begin with, one and the same. For by inheritance we simply mean, in plain English, all that is involved in the vital material which is set apart from parents to start a new life. The inheritance *is* the fertilised egg-cell, and heredity is no entity, but merely a convenient term for the relation of genetic continuity between successive generations.

But our particular point is that "Heredity," like "Horologity in clocks," like "Phlogiston" and "Caloric," and how many more "entities," has yielded before the sharpness of William of Occam's razor.

(4) Another change is marked by the more critical attitude which is now taken up in regard to various sets of facts or alleged facts relating to inheritance, which were once accepted without question. We allude to the modern criticism of alleged cases of maternal impressions, "telegony," and the transmission of acquired characters. Experience has brought home the lesson that easy-going acceptance of the first solution offered is not the scientific method. The most important line of criticism is that which has at least shaken the formerly widespread belief in the transmission of acquired characters or somatic modifications. The scepticism which Kant and Prichard and others had long before expressed was re-asserted more convincingly by Weismann in 1883 in his thesis that the child inherits from the parent *germ-cell,* rather than from the parent *body.*

Methods.—The problems of heredity have long since ceased to be studied in the arm-chair. They have been attacked precisely and practically by several distinct methods, of which the most im-

portant are (*a*) the minute study of the history of the germ-cells by which life is continued from generation to generation; (*b*) the statistical study of the measurable characters of successive generations; and (*c*) the testing of various conclusions by experimental breeding. The first may be illustrated by reference to Weismann's *Germ-Plasm* (1893) and Wilson's *The Cell in Development and Inheritance* (2nd ed., 1900); the second by Galton's *Natural Inheritance* (1889) and Karl Pearson's memoirs; and the third by Professor Cossar Ewart's *Penycuik Experiments* (1899).

Facts of Inheritance.—We do not propose to expound the facts of inheritance, but merely to indicate the present position of biology by a brief reference.

(I.) *The physical basis of inheritance* is in the fertilised ovum. Since the egg-cell is often microscopic and the sperm cell may be only $\frac{1}{100,000}$ of the ovum's size, it seems to many difficult to conceive how there can be room in these minute elements for the complexity of organisation supposed to be requisite; and the difficulty will be increased if the current opinion be accepted that only the nuclei within the germ-cells are the true bearers of the hereditary qualities. It must be at once admitted that it *is* quite impossible to form any mental picture of the fact which the word potentiality implies.

To the question: What accounts for the potentiality of the germ-cell,—what makes it, in contrast to any other cell, able to develop into an organism?—only two plausible answers have been given. To the preformationists, no objective answer was forthcoming, and the majority fell back upon a hypothesis of hyperphysical agencies.

The first attempt at an objective answer is expressed in a theory which seems to have occurred at intervals throughout the centuries, the theory of pangenesis. It was hinted at by Democritus, Hippocrates, Paracelsus, Maupertuis, and Buffon. It was suggested as a provisional hypothesis by Darwin and also by Spencer (1864). According to the theory of pangenesis, the cells of the body are supposed to give off characteristic and representative gemmules, these are supposed to find their way to the reproductive elements, which thus come to contain, as it were, concentrated samples of the different components of the body, and are therefore able to develop into an offspring like the parent. The theory involves many hypotheses, and is avowedly unverifiable in direct sense-experience, but the same might be said about many other theories. It is perhaps more to the point to notice that there is another theory of heredity which is, on the whole, simpler, which seems, on the whole, to fit the facts better, especially the fact that our experience does not warrant the conclusion that the modifications or acquired characters of the body of the parent affect in any specific and representative way the inheritance of the offspring.

As we have already hinted, the view which many, if not most biologists now take of the uniqueness of the germ-cells is rather different from that of pangenesis. It is expressed in the phrase "germinal continuity," and was suggested by several thinkers —Owen, Haeckel, Jaeger, Brooks, Galton, and Nussbaum—before Weismann worked it out into a consistent theory. In many cases, scattered through the animal kingdom, from worms to fishes, the beginning of the lineage of germ-cells is demonstrable in very early stages before the differentiation of the

body-cells has more than begun. In the development of the threadworm of the horse, according to Boveri, the very first cleavage divides the fertilised ovum into two cells, one of which is the ancestor of all the body-cells, and the other the ancestor of all the germ-cells. In other cases, particularly among plants, the segregation of germ-cells is not demonstrable until a relatively late stage. Weismann, generalising from cases where it seems to be visibly demonstrable, maintains that in all cases the germinal material which starts an offspring, owes its virtue to being materially continuous with the germinal material from which the parent or parents arose. But it is not on a continuous lineage of recognisable germ-cells that Weismann insists, for this is often unrecognisable, but on the continuity of the germ-plasm—that is of a specific substance of definite chemical and molecular structure which is the bearer of the hereditary qualities. In development a part of the germ-plasm "contained in the parent egg-shell is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generation." Thus the parent is rather the trustee of the germ-plasm than the producer of the child. In a new sense the child is a chip of the old block.

While early segregation of the germ-cells is in many cases an observable fact—and doubtless the list of such cases will be added to—the conception of a germ-plasm is hypothetical, just as the conception of a specific living stuff or protoplasm is hypothetical. We cannot demonstrate the germ-plasm, even if we may assume that it has its physical basis in the stainable nuclear bodies or chromosomes. The theory has

to be judged, like all such formulæ, by its adequacy in fitting facts.

Let us suppose that the fertilised ovum has certain qualities, *a, b, c, . . . x, y, z;* it divides and re-divides, and a body is built up; the cells of this body exhibit division of labour and differentiation, losing their likeness to the ovum and to the first results of its cleavage. In some of the body-cells the qualities *a, b,* find predominant expression, in others the qualities *y, z,* and so on. But if, meanwhile, there be certain germ-cells which do not differentiate, which retain the qualities *a, b, c, . . . x, y, z,* unaltered, which keep up, as one may say figuratively, "the protoplasmic tradition," these will be in a position by and by to develop into an organism like that which bears them. Similar material to start with, similar conditions in which to develop, *therefore,* like tends to beget like. Various attempts have been made to elaborate the general idea of genetic continuity, in terms for instance of "organic memory" (Haeckel, Hering, Samuel Butler) but it is doubtful whether they have been of real service.

It may be mentioned that Jaeger, Brooks, De Vries, and others have tried to combine the modern view with a modified version of the pangenetic hypothesis.

(II.) *The dual nature of inheritance* is another great fact, though it may seem a commonplace to the superficial. Apart from exceptional cases (asexual multiplication, parthenogenesis, and autogamy), the inheritance of every multicellular plant or animal is dual, part of it comes from the mother in the ovum or ovum-nucleus, part of it comes from the father in the spermatozoon or sperm-nucleus; the beginning of the new individuality is a fertilised

egg-cell in which two organisations are subtly mingled. We have already referred to the interesting fact that the partition of paternal and maternal chromatin-contributions between the daughter cells of the segmenting ovum can be *demonstrated* in early stages of development.

In regard to this fact of dual inheritance, three saving-clauses are suggested by recent researches.

(*a*) Although inheritance is dual, it is in quite as real a sense multiple, *from ancestors through parents*, as we shall afterwards see. (*b*) If Loeb is able to induce artificial parthenogenesis in sea-urchins' eggs exposed for a couple of hours to sea-water to which some magnesium chloride has been added; if Delage is able to fertilise and to rear normal larvæ from non-nucleated ovum-fragments of sea-urchin, worm and mollusc, we should be chary of committing ourselves definitely to the conclusion that the nuclei are the exclusive bearers of the hereditary qualities, or that both must be present in all cases. Furthermore, the fact that an ovum without any sperm-nucleus, or an ovum-fragment without any but a sperm-nucleus, can develop into a normal larva points to the conclusion, probable also on other grounds, *that each germ-cell, whether ovum or spermatozoon, bears a complete equipment of hereditary qualities.* (*c*) It must be carefully observed that our second fact does not imply that the dual nature of inheritance must be patent in the full-grown offspring, for hereditary resemblance is often strangely unilateral, the characters of one parent being "prepotent" as we say, over those of another.

(III.) *Although specific inheritance tends to be approximately complete, there are many degrees in the completeness with which an inheritance is ex-*

pressed. It will be granted by all that the completeness with which the characters of race, genus, species, and stock are reproduced generation after generation, is one of the large facts of inheritance. But it is obvious that this does not sum up our experience. The familiar saying, "like begets like," should rather read, "like tends to beget like," for variation is a more frequent occurrence than complete hereditary resemblance. An offspring cannot be a facsimile reproduction of both its parents. If it seem to show no characteristic which its parents did not between them possess, this may be due to absence of variation, or, what comes almost to the same thing, to completeness of inheritance, but it is more likely that the apparent completeness of resemblance is a fallacious inference due to our inability to detect the idiosyncrasies.

The popular platitude, "the child is a chip of the old block," will not suffice; there are some characters, e.g., tendencies to certain diseased conditions, which are more frequently transmitted than others, and the student of inheritance must work towards precise statistics of the probabilities of transmission; there are some subtle qualities whose heritability must not be assumed without evidence, thus it is of great importance to students of organic evolution that Prof. Karl Pearson has recently supplied, for certain cases, definite proof of the inheritance of fecundity, fertility, and longevity.

Before we notice some of the common modes of inheritance, we must emphasize a preliminary consideration. It is a matter of observation that there are great differences in the degree in which offspring resemble their parents; but it is a matter of conjecture that lack of resemblance is necessarily due to

incompleteness in the inheritance. Indeed, the fact that resemblance so often reappears in the third generation, makes it probable that the incompleteness is not in the inheritance but simply in its expression. The characters which seem to be absent, to "skip a generation," as we say, are probably part of the inheritance, as usual. But they remain latent, neutralised, silenced (we can only use metaphors) by other characters, or else unexpressed because of the absence of the appropriate stimulus.

The three most frequent modes of inheritance are, for convenience, called—*blended, exclusive,* and *particulate.*

(*a*) In *blended* inheritance, the characters of the two parents, e.g., in regard to a particular structure, such as the colour of the hair, are intimately combined in the offspring. This is particularly well seen in some hybrids, where the offspring seems like the mean of the two parents; it is probably the most frequent mode of inheritance.

(*b*) In *exclusive* inheritance, the expression of maternal or of paternal characters in relation to a given structure, such as eye-colour, is suppressed. Sometimes the unilateral resemblance is very pronounced, and we say that the boy is "the very image of his father," or the daughter "her mother over again"; though even more frequently the resemblance seems "crossed," the son taking after the mother, and the daughter after the father.

(*c*) It seems convenient to have a third category for cases where there is neither blending nor exclusiveness, but where in the expression of a given character, part is wholly paternal and part wholly maternal. This is called *particulate* inheritance. Thus, an English sheep-dog may have a paternal eye

on one side, and a maternal eye on the other. Suppose the parents of a foal to be markedly light and dark in colour; if the foal is light brown the inheritance in that respect is blended, if light or dark it is exclusive, if piebald it is particulate. In the last case there is in the same character an exclusive inheritance from both parents.

(IV.) *Regression.*—To Mr. Francis Galton especially we owe an analysis of the fact which stares us in the face that there is a sensible stability of type from generation to generation. "The large," he says, "do not always beget the large, nor the small the small; but yet the observed proportion between the large and the small, in each degree of size and in every quality, hardly varies from one generation to another." In other words, there is a tendency to keep up a specific average. This may be partly due to the action of natural elimination, weeding out abnormalities, often before they are born. But it is to be primarily accounted for by what Mr. Galton calls the fact of "filial regression." Let us take an instance from Mr. Pearson's *Grammar of Science.* Take fathers of stature 72 inches, the mean height of their sons is 70.8, we have a regression towards the mean of the general population. On the other hand, fathers with a mean height of 66 inches give a group of sons of mean height 68.3 inches, again nearer the mean. "The father with a great excess of the character contributes sons with an excess, but a less excess of it; the father with a great defect of the character contributes sons with a defect, but less of it."

As Mr. Galton puts it, society moves as a vast fraternity. The sustaining of the specific average is certainly not due to each individual leaving his

like behind him, for we all know that this is not the case. It is due to a regression which tends to bring the offspring of extraordinary parents nearer the average of the stock. In other words, children tend to differ less from mediocrity than their parents.

This big average fact is to be accounted for in terms of that genetic continuity which makes an inheritance not dual, but multiple. "A man," says Mr. Pearson, "is not only the product of his father, but of all his past ancestry, and unless very careful selection has taken place, the mean of that ancestry is probably not far from that of the general population. In the tenth generation a man has (theoretically) 1024 tenth great-grandparents. He is eventually the product of a population of this size, and their mean can hardly differ from that of the general population. It is the heavy weight of this mediocre ancestry which causes the son of an exceptional father to regress towards the general population mean; it is the balance of this sturdy commonplaceness which enables the son of a degenerate father to escape the whole burden of the parental ill."

At this point one should discuss reversion or atavism, but it is exceedingly difficult to get a firm basis of fact. The term reversion is here used to include cases where *through inheritance* there reappears in an individual some character which was not expressed in his parents, but which did occur in an ancestor. It includes abnormal as well as normal characters, and even the reappearance of characters, the normal occurrence of which is outside of the limits of the race altogether, i.e., in some phyletically older race. In other words, the character whose reappearance is called a reversion may be

found within the verifiable family, within the breed, within the species, or even in a presumed ancestral species.

The best illustrations of reversion are furnished by hybrids. Thus in one of Prof. Cossar Ewart's experiments a pure white fantail cock pigeon, of old-established breed, which in colour had proved itself prepotent over a blue pouter, was mated with a cross previously made between an owl and an archangel, which was far more of an owl than an archangel. The result was a couple of fantail-owl-archangel crosses, one resembling the Shetland rock-pigeon, and the other the blue rock of India. Not only in colour, but in shape, attitude, and movements there was an almost complete reversion to the form which is believed to be ancestral to all the domestic pigeons. The only marked difference was a slight arching of the tail. Similar results were got with fowls and rabbits.

Such facts lead us to the theory that characters may lie latent for a generation or for generations, or in other words that certain potentialities or initiatives which form part of the heritage may remain unexpressed for lack of the appropriate liberating stimulus, or for other reasons, or may have their normal expressions disguised. But it does not follow that the reappearance of an ancestral character not seen in the parents is necessarily due to the reassertion of latent elements in the inheritance. It may be a case of ordinary regression; it may be a case of arrested development; it may be an extreme variation whose resemblance to an ancestral characteristic is a coincidence; it may be an individually acquired modification, reproduced apart from inherit-

ance, by a recurrence of suitable external conditions, and so on. In short, what are called reversions are properly in many cases misinterpretations.

(V.) *Galton's Law.*—The most important general conclusion which has yet been reached in regard to inheritance is formulated in Galton's Law. Mr. Galton was led to it by his studies on the inheritance of human qualities, and more particularly by a series of studies on Basset hounds. It is one of those general conclusions which have been reached statistically, and we must refer for the evidence and also for its strictest formulation to the revised edition of Mr. Pearson's *Grammar of Science.*

As we have seen, it is useful to speak of a heritage as dual, half derived from the father and half from the mother. But the heritable material handed on from each parent was also dual, being derived from the grandparents. And so on, backwards. We thus reach the idea that a heritage is not merely dual, but in a deeper sense multiple.

According to Galton's law, "the two parents between them contribute *on the average* one-half of each inherited faculty, each of them contributing one-quarter of it. The four grandparents contribute between them one-quarter, or each of them one-sixteenth; and so on, the sum of the series $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16}$ + etc., being equal to 1, as it should be. It is a property of this infinite series that each term is equal to the sum of all those that follow: thus $\frac{1}{2} = \frac{1}{4} + \frac{1}{8} + \frac{1}{16}$ + etc., $\frac{1}{4} = \frac{1}{8} + \frac{1}{16}$ + etc., and so on. The prepotencies or subpotencies of particular ancestors, in any given pedigree, are eliminated by a law that deals only with *average* contributions, and the varying prepotencies of sex in respect to different qualities are also presumably eliminated."

Transmissibility of Acquired Characters or Modifications.—Since 1883, when Weismann expressed his entire scepticism as to the transmission of acquired characters, the question has been almost continuously debated. This is not surprising, for it is much more than a technical problem for biologists. It is of profound interest to the parent, the physician, the teacher, the moralist, and the social reformer; and it really concerns us all, for the answer to it affects every-day conduct. This is sufficient reason for devoting some attention to it here, and this is further justified by the fact that although the negative position has been tentatively assumed at various periods, e.g., by Kant and by Prichard (b. 1786), the careful discussion of the question is characteristic of the last quarter of the nineteenth century, and dates from an essay by Galton in 1875,* and from one by Weismann in 1883.†

"*Modifications*" *or* "*Acquired Characters*" may be defined as structural changes in the body of the organism induced by changes in the environment or in the function, and such that they transcend the limit of organic elasticity, and therefore persist. Plants of the plain when brought into Alpine conditions may develop more protective tissue and exhibit many other modifications. The white man who works for many years under a tropical sun may become so deeply tanned that the result does not disappear after years of residence in Britain. Unlike the Ethiopian he has changed his skin, but he cannot change it back again. Through prolonged disuse

* *A Theory of Heredity*, Contemporary Review, XXVII., pp. 80–95.

† *Ueber die Vererbung*, Jena, trans. Oxford, 1889.

from early years onwards a muscle may pass into a state of atrophy, through prolonged exercise another may become exaggerated, and the modifications in either case may last a lifetime. Endless examples might be given.

But to understand the matter more clearly we must contrast "modifications" due to "nurture" with "variations" due to "nature." When we compare living creatures of the same kind, children with parents, brother with brother, neighbour with neighbour, native with foreigner, we recognise that there are many differences between them, though they all fall within the range which we call "the same species." To begin with, we call these *the observed differences* between individuals. As we come to analyse them, however, we discern that a number are definitely associated with particular functions and surrounding influences. They may not be hinted at in the young forms, but they begin to appear when the particular conditions begin to operate. They can be definitely related to some alteration or difference in environment or in function, and they are usually exhibited in some degree by all organisms of the same kind which are subjected to the same change of conditions. These we call "*modifications*" or acquired characters. Now when we subtract from the total of observed differences the modifications which we have detected, there remain a number of differences which we call "*variations*." We cannot causally relate them to differences in habit or surroundings, they are often hinted at even before birth, and they are not alike even among forms whose conditions of life seem absolutely uniform. We suppose that they have an origin in changes of the germinal material before or after fertilisation; we

call them congenital or germinal variations, and there is no doubt that they are transmissible. The precise problem is, whether the modifications of the body can so specifically affect the reproductive cells that the next generation will inherit in some measure the modification acquired by the parent or parents. If summing up, in Galton's phrase, we call the effects of surrounding influences "*nurture*," our question is seen to be an extraordinarily important one, May the results of nurture be transmitted, or is it the "*nature*" alone that constitutes the inheritance?

Widespread Opinion in Favour of Affirmative Answer.—In fairness we are bound to recognise that the verdict of the *practical* man, whether gardener or farmer, breeder or physician, is still predominantly in favour of an affirmative answer.

There is little to be gained by a citation of opinions, there are equally great names on both sides. It cannot be an easy question when we find Spencer on one side and Weismann on the other, Haeckel on one side and Professor Ray Lankester on the other, Sir William Turner on one side and Professor His on the other, and so on.

The reason why the affirmative position is so widely held is probably threefold: (1) First, that there are many facts which suggest modification-inheritance until they are examined critically. The late Duke of Argyll said that the world is strewn with illustrations, and Dr. Haacke has compared the evidence for the affirmative to the sand on the seashore for multitude, yet neither furnishes us with a single grain which will bear analysis. That it is an obvious interpretation we grant, but the obvious

interpretation is seldom the right one. The sun does not go round the earth. (2) Second, it is an interpretation which would seem to make the theory of organic evolution simpler; it suggests a more direct and rapid method than the natural selection of congenital variations. If to a growing and varying nature or congenital inheritance there be continually added the results of nurture, the rate of evolution would be quickened both upwards and downwards. Our first business, however, is to find out whether the hypothesis actually consists with experience. (3) Third, we are so accustomed in human affairs to the entailment of gains from generation to generation, to standing on the shoulders of our ancestors' achievements, that it seems difficult for some to refrain from projecting this on organic nature, forgetful of the fact that the greater part of our entailing process is altogether apart from organic inheritance. It comes about through *social inheritance* embodied in tradition, convention, institution, literature, art, law, etc., of which there are among animals only vague analogues.

A General Argument Against.—Apart from the fact that he found the evidence brought forward in favour of the belief in the inheritance of acquired characters to be "a handful of anecdotes," Professor Weismann was led to his position of extreme scepticism by his realisation of the continuity of generations.

It is evident that if the germ-plasm or the material basis of inheritance be something apart from the general life of the body, sometimes set apart from a very early stage, there is a presumption against the likelihood of its being readily affected in a specific manner by changes in the nature of the body-cells.

The germ-plasm is in a sense so apart that it is difficult to conceive of the mechanism by which it might be influenced in a specific or representative manner by changes in the cells of the body.

A General Argument For.—We have recognised that the germ-cells may be early set apart in the building up of the body, and that they sometimes seem scarcely to share at all in its daily life. On the other hand, in many plants the distinction between body and germ-cells can hardly be drawn, and even if we keep to animals the bonds between the body and its germ-cells are often very close. The blood and lymph or other body fluids form a common medium for all the parts of the animal; alteration of diet in the early youth of some animals like tadpoles and caterpillars may determine the predominance of one sex or the other through influences which must pass from body to germ-cells; various poisons may affect the whole bodily system and the germ-cells as well, and there are real though dimly understood correlations between the reproductive system and the rest of the body. It is therefore erroneous to think of the germ-cells as if they led a charmed life uninfluenced by any of the accidents and incidents in the daily life of the body which bears them. No one believes this, Weismann least of all, for he finds one of the chief sources of congenital variation in the nutritive stimuli exerted on the germ-plasm by the varying state of the body.

There are some who find in this "a concealed abandonment of the central position of Weismann," and one of them has recently put the argument thus: if the germ-plasm is affected by changes in nutrition in the body, and if acquired characters affect changes in nutrition, then "acquired characters or their con-

sequences will be inherited." But it is quite illegitimate to slump "acquired characters and their consequences" as if the distinction was immaterial. The illustrious author of the *Germ-Plasm* has made it quite clear that there is a very great difference between admitting that the germ-plasm has no charmed life, insulated from bodily influences, and admitting the transmissibility of *a particular acquired character,* even in the faintest degree. The point, let us repeat, is this: Does a change in the body, induced by use or disuse or by a change in surroundings, influence the germ-plasm in such a specific or representative way that the offspring will exhibit the same modification which the parent acquired or even a tendency towards it?

Even when we fully recognise the unity of the organism, that each part shares in the life of the whole, it is very difficult for those who accept the belief in the inheritance of acquired characters to suggest any *modus operandi* whereby a particular modification in the brain or the little toe, the root or the petal, can specifically affect the germinal material in such a way that the modification or a tendency towards it becomes part of the inheritance. Did we accept Darwin's provisional hypothesis of pangenesis according to which the parts of the body give off gemmules which are carried as samples to the germ-cells, the possibility of transfer might seem more intelligible. But Darwin's suggestion remains a pure hypothesis, and is accepted by none except in extremely modified form. Indeed it may be recalled that it was the failure of his attempt to find confirmation of Darwin's hypothesis by experiments on the transfusion of blood which led Galton many years ago to doubt whether there was any inheritance of

acquired characters. Yet, in fairness, we must note how little we understand the influences which pass in the other direction from reproductive organs to body, and recall Lloyd Morgan's warning that although we cannot conceive how a modification might as such saturate from body to germ-cells, this does not exclude the possibility that it may actually do so.

Particular Evidence For.—Let us now give a few examples of the particular or *a posteriori* evidence in favour of the inheritance of acquired characters, and to suggest some of the difficulties which rob the evidence of cogency.

It has been stated that the Panjabis of India show certain peculiarities of musculature and skeleton which are plainly related to the frequency with which these people assume on all possible occasions the squatting posture. Like so many other pieces of so-called evidence this does not tell one enough, e.g., whether the peculiarities are seen on new-born Panjabi babes, and whether the peculiarities appear to be on an increase. As it stands, the evidence is quite inconclusive, and we may place against it the case of the compressed foot of Chinese ladies—in regard to which we have likewise few satisfactory details, but certainly not as yet any evidence that the long-continued deformation has resulted in any hereditary change in the Chinese baby's foot. The alleged dwindling of the little toe has been impetuously instanced as a case in point—as a case of the inheritance of a modification produced by tight boots. But there is no satisfactory evidence; a dwindling has also been alleged in savages who do not wear boots; it is possible that there is in man as there was in the horse a congenital variation in favour of reduction

of digits; and there are other possible explanations.

About a hundred years ago (1796), an authority on trotting horses stated that the utmost speed of the English trotter was a mile in 2 minutes, 57 seconds. Since 1818, accurate trotting records have been kept, and an inspection of these shows that very gradually, decade after decade, the speed and the percentage of swift trotters increased. Finally there has been evolved a breed who can trot the mile in 2 minutes, 10 seconds. It is claimed by Cope and others that we have here evidence of the cumulative transmission of the results of exercise or nurture. But a sceptical consideration leads one to doubt if the case is even relevant; the interpretation in terms of use-inheritance overlooks the results of selective breeding which may have increased the congenital swiftness, and the process of elimination which persistently weeded out the less swift from the stud.

Reference is often made in biological literature to the observations and experiments of Schmankewitsch in 1875 on certain brine-shrimps belonging to the genus *Artemia*. By lessening the salinity of the water he was able to transform one type, *Artemia salina,* in the course of generations into another type, *Artemia milhausenii;* and conversely, by increasing the salinity. Although he did not himself make any such claim, his work has often been referred to as an illustration of changing one species into another. It had indeed the undeniable result of showing that certain forms of life are very plastic, even to such influences as altered salinity. Apart altogether from the criticism of experts, which has been damaging, it may be recognised that Schmankewitsch experimented with *a progressively changing environment* on a series of generations, and that the

result is readily interpretable as due to cumulative modifications hammered on each successive generation without there being any inheritance of these modifications. It is also possible that the reproductive cells were influenced along with the body or outside of the body by the continuous change of salinity.

Another typical line of evidence is that based on the study of immunity—a subject of great practical importance and theoretical interest. A due discussion of it is impossible in our space here, but the particular point admits of being briefly stated. It is well known that negroes and mongolians are *relatively* immune to yellow fever, and it is believed by many that a progressive immunity to various diseases is observable in our own country. Is not this proof positive of the inheritance of an acquired character? The sceptical answer is first of all that the original immunity may have been a congenital peculiarity, which has become dominant in the race by the elimination of those members who were not immune. And if it be objected that there are cases where a mother-rabbit or guinea-pig has been artificially rendered immune to certain diseases, and has had young ones born immune, the answer is again ready, that this was probably due to a kind of infection before birth, some anti-toxin or other having probably passed from the mother to the unborn young.

Indirect Importance of Modifications.—That modifications are common, everyone admits; that they are often of great value to the individuals who acquire them is also certain; the question is whether they are of direct value to the race, seeing that we cannot prove their transmissibility.

In this connection a recent suggestion of much interest has been made by Professors Mark Baldwin, Lloyd Morgan, and Osborn, namely, that adaptive modifications may act as the fostering nurses of congenital variations in the same direction. An illustration will make the general idea clear.

Let us suppose a country in which a change of climate made it year by year of the utmost importance that the inhabitants should become swarthy. Some individuals with a strong natural or congenital tendency in this direction would doubtless exist, and on them and their similarly endowed progeny the permanent success of the race might wholly depend. On the other hand, there might be many individuals in whom the constitutional tendency in the direction of swarthiness was too weak and incipient to be of use. If these, however, made up for their lack of natural swarthiness by a great susceptibility to acquired swarthiness, it is conceivable that the modification, though never taking organic root, would serve as a life-saving screen until coincident congenital variations in the direction of swarthiness had time to grow strong.

Practical Conclusions.—It seems then that the scientific position at present should be one of active scepticism—leading on to experiment. It also seems to us necessary at present to give a verdict of non-proven for the affirmative, with a strong presumption in favour of the negative answer.

If this be so, how should the scientific position react upon conduct? Supposing that the negative be the answer, what should be our attitude to education, physical culture, amelioration of function, improvement of environment, and the like? There can be no doubt that these should become increasingly im-

portant in our eyes. If the results of nurture are not inherited, it is all the more urgent that we should secure that the influences making for evolution should be brought to bear upon each successive generation. "Is my grandfather's environment not my heredity?" the American asks. Well, if not, let me secure my grandfather's environment if it made for progress, and flee from it if it tended elsewhere. Is nurture not inherited?—perhaps it is just as well, for we are novices at nurturing even yet. Is nature alone inherited?—then we are saved from undue pessimism when we think of the harmful functions and environments which disfigure our civilisation. Is there not some result if we are forced to the conviction that, to sustain and improve the standard of our race, we must bend our energies more and more to the development (in the true sense) of our function and environment. At the same time, there is no denying the thought that man is a slowly reproducing, slowly varying organism, and that for progress which is really organic—progress that is in nature—we must wait patiently.

On the negative side—of inaction—the scientific decision ought, however, to have some effect. No longer should we hear the still frequent assertion: "Ah, he has got his father's nature, it does not matter much what he learns, or what he does, or where he lives, he will come all right out of it," forgetting that what is called the father's nature is much more than his inheritance, it is in adult life the inheritance plus all the results of acquired characters. No longer should we hear the extreme pessimism in regard to the decadence—the débâcle—the abyss—towards which those who fix their attention on the disagreeable acquired characters of our

age think we are fast hastening, for there is at least something to be said biologically for the view that these are but transient acquired characters, like loathsome paint on sound British oak. The veneer on the little boy pictured at the beginning of *Captains Courageous* was odious, but it soon peeled off on the Cod-Banks, where an appropriate nurture—both functional and environmental—allowed the constitutional worth to realise itself.

If there is little scientific warrant for our being other than sceptical at present as to the transmission of acquired characters, this scepticism lends greater importance than ever, on the one hand, to a good "nature," to secure which is the business of careful mating; and, on the other hand, to a good "nurture," to secure which for our children is one of our most obvious duties, the hopefulness of the task resting upon the fact that, unlike the beasts that perish, man has a lasting external heritage, capable of endless modification for the better, a heritage of ideas and ideals embodied in prose and verse, in statue and painting, in Cathedral and University, in tradition and convention, and above all in society itself.

CHAPTER XI.

THE THEORY OF ORGANIC EVOLUTION.

THE general idea of evolution, like many other great ideas, is essentially simple—that the present is the child of the past and the parent of the future. It is the same as the scientific conception of human history. In human affairs, what seems to the careless to be quite new is revealed to the student as an antiquity. We see the gradual growth of social organisations, the natural transition from one established order of things to another slightly different, the transformation of one institution into another, and we formulate the growth, the transition, the transformation in the general concept of historic evolution. A process of Becoming leads to a new phase of Being; the study of evolution is a study of *Werden und Vergehen.*

THE GENERAL IDEA OF ORGANIC EVOLUTION.

Stated concretely in regard to living creatures, the general doctrine of organic evolution suggests, as we all know, that the plants and animals now around us are the results of natural processes of growth and change working throughout the ages, that the forms we see are the lineal descendants of ancestors on the whole somewhat simpler, that these are descended

from yet simpler forms, and so on backwards, till we lose our clue in the unknown—but doubtless momentous—vital events of pre-Cambrian ages, or, in other words, in the thick mist of life's beginnings.

HISTORY OF THE EVOLUTION-IDEA.

"Though the general idea of organic evolution is simple, it has been slowly evolved, gaining content as research furnished fuller illustration, and gaining clearness as criticism forced it to keep in touch with facts. It has slowly developed from the stage of suggestion to the stage of verification; from being an *a priori* anticipation it has become an interpretation of nature; and from being a modal interpretation it is advancing to the rank of a causal theory." *

(1) In what we may call "the Greek Period," there were many who more or less vaguely suggested the evolution-idea, notably Empedocles (495–435 B.C.). Aristotle (384–322 B.C.) speaks clearly of a gradual progression in nature from the inorganic to the organic and from one grade of life to another.† From Epicurus (341–270 B.C.), the first poet of evolution, we pass after a long interval to Lucretius (99–55 B.C.).

(2) In the mediæval period, though there was a general arrest of enquiry, the light of the evolution-idea did not wholly die. Bruno (1548–1600) at least, who proclaimed that "the investigation of Nature in the unbiased light of reason is our only guide to truth," was in some degree an evolutionist.

* See the writer's *Science of Life*, 1899 p. 213, where this section forms the subject of a whole chapter "The Evolution of Evolution-Theory."

† See E. Clodd, *Pioneers of Evolution* (1897); H. F. Osborn, *From the Greeks to Darwin* (1894).

(3) As the result of the scientific renaissance in the seventeenth century, when science re-asserted itself as a natural expression and discipline of the developing human spirit, the evolution-idea became clear to many minds. Professor Osborn notes that the philosophers, rather than the naturalists, were "upon the main track of modern thought." Descartes (1596–1650) and Leibnitz (1640–1716) point onwards to Spinoza and Hume, Lessing and Schelling, Kant and Herder. On another line we have Francis Bacon (1561–1626), clearly evolutionist in his outlook.

In the eighteenth century there were not a few "speculative evolutionists," as Osborn calls them, such as De Maillet, Maupertuis, Diderot, and Bonnet, whose methods were wrong, though their ideas were often right. Many say that the same title must also be applied to Lorenz Oken (1776–1851).

(4) As undoubted pioneers of modern evolution-doctrine we must rank Buffon (1707–1788), Erasmus Darwin (1731–1802), Lamarck (1744–1829), Goethe (1749–1832), Treviranus (1776–1837), Étienne Geoffroy Saint-Hilaire (1772–1844), and Robert Chambers (1802–1871); and there are others of whom a complete history should take notice. We have elsewhere given brief summaries of the characteristic views of the pioneers.*

(5) It may be said that Darwin did three chief services to evolution-doctrine. (*a*) "By his patient, scholarly, and pre-eminently fair-minded marshalling of the so-called 'evidences' which suggest the doctrine of descent, he won the conviction of the biological world. He made the old idea current intellectual coin. In so doing he was greatly aided by

* *Science of Life*, 1899, pp. 219-223.

Spencer and Wallace, Haeckel and Huxley. (*b*) He applied the evolution-idea to various sets of facts, such as the expression of the emotions and the descent of man, and showed what a powerful organon it was. Here, again, he was greatly aided by his contemporaries, and Spencer's work in this direction is even more important than Darwin's. (*c*) At the same time as Alfred Russel Wallace, he elaborated the theory of natural selection, of which there had been a few previous suggestions." *

(6) Since Darwin secured the general acceptance of the evolution-idea, the attention of evolutionists has been chiefly directed to a discussion and criticism of the factors in the evolution-process. Natural Selection working on germinal variations has seemed to some an adequate formula; and this consistent Darwinism had been strengthened by a recognition of the importance of Isolation (Romanes and Gulick), while Weismann has added the subtle idea of "Germinal Selection." In spite of the growing scepticism as to the transmissibility of functional and environmental modifications, many adhere to the Lamarckian and Buffonian position, that these are of direct importance in evolution. This may or may not be combined with a recognition of the importance of Selection. Others, again, following Goethe and Nägeli, regard the evolution of organisms as pre-eminently a story of self-differentiating and self-integrating growth,—cumulative, selective, definite, and harmonious like crystallisation. Believing in progressive variations in definite directions as opposed to indefinite sports, they find little need to invoke Natural Selection except as pruning the occa-

* *Op. cit.* p. 223.

sional exuberances of the *arbor vitæ*. Thus we have Darwinian, Lamarckian, and Nägelian schools, and various combinations of these up to complete eclecticism. From this others have reacted to an agnostic position, which in its more kinetic expression means *active scepticism*, and this *thätige Skepsis* seems to us the more useful mood for present-day evolutionists.

SUMMARY.—*The evolution-idea is not only essentially simple, but also very ancient. It is perhaps as old as clear thinking, which we may date from the (unknown) time when man discovered the year—with its marvellous object-lesson of recurrent sequences,—and realised that his race had a history. Whatever may have been its origin, the idea was familiar to several of the ancient Greek philosophers, as it was to Hume and to Kant; it fired the imagination of Lucretius and linked him to another poet of evolution—Goethe; it persisted, like a latent germ, through the centuries of other than scientific pre-occupation; it was made actual by the pioneers of modern biology—men like Buffon, Lamarck, Erasmus Darwin, and Treviranus;—and it became current intellectual coin when Darwin, Wallace, Spencer, Haeckel, and Huxley, with united but varied achievements, won the conviction of the majority of thoughtful men. Since this achievement, there has been a concentration of enquiry on the originative and directive factors in the evolution-process, but this enquiry is still young.*

THE PRESENT ASPECT OF THE EVOLUTION THEORY.

Attitude towards the General Idea of Evolution.—The appreciation of the general idea of evolution has changed for the better since the early Darwinian

days of hot-blooded controversy. It seems to be generally recognised, for instance, that the evolution formula is not antithetic to transcendental formulæ. The Theory of Descent tacitly makes the assumption —the basal hope of all biology—that it is not only legitimate but promiseful to try to interpret scientifically the history of life upon the earth. If we have good reasons for believing that the long process of Becoming which has led eventually to ourselves and our complex animate environment is altogether too mysterious or too marvellous to admit of successful treatment by ordinary scientific methods, then we deny at the outset the validity of the evolution formula.

Here is a parting of the ways, and there is no *via media*. Is there no hopefulness in attempting this scientific analysis of the confessedly vast and perplexing problem?—then let us remain poets and artists, philosophers and theologians, and sigh over a science which started so much in debt that its bankruptcy was a foregone conclusion. On the other hand, if the scientific attempt is legitimate, and if it has already made good progress, considering its youth, then let us rigidly exclude from our science all other than scientific interpretations; let us cease to juggle with words in attempting a mongrel mixture of scientific and transcendental formulation; let us stop trying to eke out demonstrable factors by assuming, alongside of these, "ultra-scientific causes," "spiritual influxes," *et hoc genus omne:* let us cease writing or buying books such as *God or Natural Selection,* whose titular false antinomy is an index of their misunderstanding. Not that we are objecting for a moment to any metaphysical or theological interpretations whatsoever; we are simply

emphasising the so much neglected commonplace that we cannot have scientific formulæ mixed up with any other interpretations in one sentence; and that to place these other interpretations *in opposition to* scientific formulæ is to oppose incommensurables, and to display an ignorance of what the aim of science is.

From the Fact to the Factors.—So far then the formula, but let us pass to the more difficult question of the *factors*. Evolution is a certain mode of becoming, what are the operative conditions? Here we pass from practical certainty to perplexing uncertainty, as is so often the case when we pass from the general to the particular, from abstract to concrete.

Nature of Variations.—The first great question is as to what may be called the raw materials of progress,—the origin and nature of those variations or organic changes on which the possibility of evolution depends.

Darwin started from the broad fact that variability exists (illustrating it chiefly from domesticated animals and cultivated plants); he postulated a crop of organic changes, both of tares and wheat; and he pointed out how a process of 'singling' and thinning, sifting and winnowing would operate upon the ever-growing, ceaselessly changing crop so that the result was progress. But all science begins with measurement, and the great step in advance that has been made of recent years is in the dry and tedious, but absolutely necessary, task of recording accurately the variations which do actually occur.

Without being biologists, simply as clear thinkers, we can see the unsatisfactoriness of the line of argument which was until recently prevalent,—that

of simply postulating variability without statistically or otherwise defining it. Life is so abundant and so Protean that biologists tend to draw cheques upon Nature as if they had unlimited credit, and in their impetuosity scarce wait to see whether these are honoured.

But we are now changing all this. From Heligoland to California, from Plymouth to Nigg, we have now reports of fundamentally important studies on variation, which are rapidly helping us out of the slough of vagueness in which, to the physicist's contempt, biology still flounders. The very title—*Biometrika*—of a new journal is a sign of the times.

It is far too soon to sum up recent studies on variation, but a few general results are becoming clear. The tiresome objector who challenges the evolutionist to demonstrate a single case of one species being turned into another, has an undeveloped "time-sense" (all natural history records embracing but a fraction of a tick of the cosmic clock); and he is a century behind the times, with an outlook like that of the catastrophic or cataclysmal school of geologists. Whoever expects to find big "Jack-in-the-box" phenomena in nature is sure to be disappointed. What the objector should do is humbly to study some of the recent researches in which the persistent patience of those who can appreciate millimetres has shown that variability is even greater than was supposed by Darwin, and is certainly not less among creatures living in a state of nature than among those domesticated or cultivated forms on which Darwin concentrated his attention. And he should at least give as many days as the observers have given years to the study of palæontological series, like those of Ammonites and

Brachiopods. The fact is that whenever we settle down to measure, describe, and identify, we find that specific diagnoses are averages; that specific characters require a curve of frequency for their expression; that the living creature is usually a Proteus. There are no doubt long-lived, non-plastic, conservative types, like Lingula, and perhaps a score of other well-known instances, where no visible variability can be proved even in millions of years, but to judge from these as to the march of evolutionary progress is like estimating the rush of a river from the eddies of a sheltered pool.

In the study of variability it seems possible to distinguish between continuous variation, in which the descendant has a little more or a little less of a given character than the parents had, and discontinuous variation, apparently frequent, in which a new combination (say, an elegant vase-like pitcher on a cabbage leaf) appears suddenly without known gradational stages and with no small degree of perfection. Though Lamarck said "Nature is never brusque," though we adhere to our statement about the rarity of big Jack-in-the-box phenomena, the evidence (e.g., of Bateson) as to the occurrence of discontinuous variations appears conclusive. Such words as "freaks" and "sports" are open to objection, but they suggest the idea of what Mr. Galton calls "transilient" variations, and the fact that organic structure may pass with seeming abruptness from one form of equilibrium to another.

It also becomes more and more evident that the living creature in many cases varies as a whole or unity, so that if there is more of one character there is less of another, and so that one change brings another in its train. If this be so, we are not restricted

to the assumption of the piecemeal variation of minute parts. It seems, according to De Vries, as if the organism as a whole—through its germinal organisation, of course—may suddenly pass from one position of organic equilibrium to another. This consideration, and actual measurement, seem also to suggest that there is a greater definiteness and a less fortuitousness in variation than was previously supposed.

Origin of Variations.—In his great work, *Materials for the Study of Variation,** Mr. Bateson devotes a line to saying that enquiry into the causes is in his judgment premature; and it must be admitted that until we know the actual facts better, we cannot expect to say much that is wise in regard to their antecedents. A number of suggestions have been made, however, and some of these may be briefly stated.

A variation, which renders the child different from its parents, is often interpretable as due to some incompleteness of inheritance or in the *expression* of the inheritance. It seems as if the entail were sometimes broken in regard to a particular characteristic. Oftener, perhaps, as the third generation shows, the inheritance has been complete enough potentially, but the young creature has been prevented from realising its entire legacy. Contrariwise, it may be that the novelty of the newborn is seen in an intensifying of the inheritance, for the contributions from the two parents may as it were corroborate one another.

But in many cases something turns up to which we irresistibly apply the word novel, some peculiar

* See Fourth Edition, 1901.

mental pattern, it may be, which we feel bound to call original, some structural change which suggests a new departure. We may tentatively interpret this as due to some fresh permutation or combination of the complex nuclear and cellular substances which are mingled at the outset of every new life sexually reproduced. The plausibility of this interpretation is increased when we remember that our inheritance, as Galton has so clearly shown, is mosaic rather than dual. For it is not merely in an intermingling of maternal and paternal contributions that life begins, but of legacies through the parents from remoter ancestors. The complexity of the problem is increased, not diminished, if there be reality in the conception that the different hereditary qualities may have a struggle *in nuce,* or that there is a "germinal selection" as Weismann calls it.

Another possibility of variation has been sought in the fact that the hereditary material is doubtless very complex and has a complex environment within the parental body. If it has, in spite of its essential stability, a tendency to instability as regards minor details, we may perhaps find the change-exciting stimuli in the ceaseless nutritive oscillations within the body. But enough has been said to indicate how uncertain is the voice of biology in answering the fundamental questions as to the nature and origin of variations.

Modifications.—Among the observed differences which mark man from man, trout from trout, buttercup from buttercup, there are many to which we cannot apply the term variations. Quite apart from constitutional or germinal changes there are differences which are obviously impressed upon the body from without, such as sun-burning, or which result

from use and disuse, such as callosities on the fingers. These do indeed presuppose a constitution capable of being changed, but we can relate each of them (sometimes with certainty, sometimes only with probability) to some definite influence either of function or of environment which has brought about a structural change transcending the limits of organic elasticity. We call these conveniently "modifications." Now, though organic "modifications" may be of much importance to the individuals possessing them, and may serve as a temporary shield for incipient variations in the same direction, they are not proved to be of any direct importance in the evolution of the race, for the simple reason that there is no convincing evidence that they can be as such or in any representative degree transmitted to the offspring.

So far then we have seen that the raw materials of evolution consist of constitutional or germinal variations, and that we are not justified in including modifications or acquired characters because their transmissibility is unproved. Let us now pass to a brief consideration of the secondary or directive factors—operating upon the variations which crop up.

Natural Selection.—The first of these directive factors is natural selection, and it is well known that the most distinctive contribution which Darwin and Wallace made to ætiology was to emphasize its importance. The theory admits of brief statement.

Variability is a fact of life, the members of a family or species are not born alike; some have qualities which give them a little advantage both as to hunger and as to love; others are relatively handicapped. But a struggle for existence is also a fact of life, being necessitated especially by two facts,

first that two parents usually produce many more than a pair of children, and the population thus tends to outrun the means of subsistence; and, secondly, because organisms are at the best only relatively well-adapted to their conditions, which, moreover, are variable. This struggle does not express itself merely as an elbowing and jostling around the platter, but at every point where the effectiveness of the response which the living creature makes to the stimuli playing upon it, is of critical moment. As Darwin said, though many seem to have forgotten, the phrase, "struggle for existence" is used "in a wide and metaphorical sense," including much more than an internecine scramble for the necessities of life,—including, indeed, all endeavours for preservation and welfare, not only of the individual, but of the offspring too. In many cases, the struggle for existence both among men and beasts is more fairly described as an endeavour after well-being, and what may have been primarily self-regarding impulses become replaced by others which are distinctively species-maintaining, the self failing to find full realisation apart from its kin and society.

Now, in this struggle for existence—manifold in its expression, but never unreal—the relatively less fit forms tend to be eliminated. This does not necessarily mean that they come at once to a violent end, as when locust devours locust or the cold decimates the birds in a single night, but often simply that the less fit die before the average time, and are less successful than their neighbours as regards offspring. But whether the eliminative process be gentle or severe, the result is the same, that the relatively more fit tend to survive; and since many variations (the argument continues) are transmitted

from generation to generation, and may through the pairing of similar or suitable mates be gradually increased in amount, the eliminative or selective process works towards the establishment of new adaptations and new species.

As to that particular form of natural selection which is called sexual selection, to which Darwin attached so much importance especially in his later work, we are compelled to shirk the discussion of a difficult problem which could not be fairly treated within our limits of space. Only a few remarks can be made. As is well known, sexual selection takes two chief forms (*a*) where the rival males fight for the possession of a desired mate or mates, and in so doing reduce the leet; and (*b*) where the females appear to choose certain individuals from amid a crowd of suitors. The general verdict seems to be that while among some animals preferential mating appears indisputable, its range and its effectiveness in evolution are much less than Darwin believed. This is well expressed in the work of Darwin's magnanimous colleague, Alfred Russel Wallace, who has given good reason for believing that too much credit has been given to this sexual selection factor. But just as the little child in a sense leads the race—being the expression of some new variation,—so we may still admit that there are facts which warrant us in saying that *das ewig weibliche* plays a part in the upward march of life. Cupid's darts as well as Death's arrows have sometimes evolutionary significance.

Apart from differences of opinion as to the importance of sexual selection, it seems fair to say that the majority of naturalists continue to rely with confidence on the general selective or eliminative

process. Whether the selection theory is "all sufficient," as Weismann calls it, or "inadequate," as Spencer says, it remains a potent theory. Given a sufficiently abundant crop of variations, a persistent struggle for existence, and a large draft on the bank of Time, what may the selective process not accomplish?

But as ætiology has grown older and wiser, it has begun to ask questions, the answers to which will mean much progress. Thus there is a demand for some serious attempt to measure the intensity of the struggle in typical cases, and for evidence that the absence of a particular variation in certain members of the stock does really determine their elimination. There are enquiries as to the frequency of discontinuous or transilient variations—where a new character is reached with apparent suddenness, for if these are frequent this may lessen the claims which have to be made on the selective process. It is asked whether the task of elimination will not be further lessened if the crop of variations is more definite and less of the nature of random freaks than used to be supposed. Information is wanted as to the degree in which the struggle for existence is directly competitive, or merely between the living creature and its inanimate surroundings. Especially is it desired that statistics be forthcoming to show how far the elimination is discriminate, as when the pruner lops off the less promiseful branches, or the breeder gets rid of the unsuitable members of his stock, and how far it is indiscriminate, as when the hastily driven hoe strikes the cluster of seedlings. In other words, evolutionists have awakened to the necessity of testing natural selection in relation to actual cases.

Isolation.—The raw materials of progress are fur-

nished, as we have seen, by constitutional or germinal variations. What these may amount to depends in the long run on the potentialities resident in living matter, especially of reacting to external influences, and this forces us finally back to the institution of the order of nature which at some level or other the evolutionist takes for granted. In organic evolution, variation supplies the materials; heredity (or the relation of genetic continuity between successive generations) is one of the conditions; natural selection or elimination is one of the directive factors. But there may be others, and one has been indicated in what is called the theory of isolation.

A formidable objection to the Darwinian theory, first clearly stated by Professor Fleeming Jenkin, and familiar to everyone who has thought out the matter, is that variations of small amount and sparse occurrence would tend to be swamped out by intercrossing. In artificial selection, the breeder takes measures to prevent this by pairing similar or suitable forms together; but what in nature corresponds to the breeder?

Various suggestions have been made in answer to this question. Thus Professor Weismann says, "The necessary variations from which transformations arise must in all cases be exhibited over and over again by many individuals," but there is still a lack of concrete evidence to bear this out. We do not mean to deny it, but before we lean heavily upon it we should like to be able to furnish numerous examples of many similar variations occurring at once within the same group.

The favourite answer of recent years is that worked out by the late Dr. Romanes, Mr. Gulick, and others—the theory of isolation. They point to

the great variety of ways in which, in the course of nature, the range of intercrossing is restricted—e.g., by geographical barriers, by differences in habit, by psychical likes and dislikes, by reproductive variation causing mutual sterility between two sections of a species living on a common area, and so on. According to Romanes, "without isolation, or the prevention of free inter-crossing, organic evolution is in no case possible." Again it has to be confessed that the body of facts in illustration of isolation and its effects is unsatisfactorily small.

An interesting corollary has been recently indicated by Professor Cossar Ewart.* Breeding within a narrow range often occurs in nature, being necessitated by geographical or other barriers. In artificial conditions, this in-breeding often results in the development of what is called prepotency. This means that certain forms have an unusual power of transmitting their peculiarities, even when mated with dissimilar forms. In other words, certain variations have a strong power of persistence. Therefore, wherever through in-breeding (which implies isolation) prepotency has developed, there is no difficulty in understanding how even a small idiosyncrasy may come to stay, even although the bridegroom does not meet a bride endowed with a peculiarity like his own.

In Conclusion.—In conclusion, or we should rather say in ending this review whose point is its inconclusiveness, let us once more emphasise that while the general idea of evolution stands more firmly than ever as a reasonable modal interpretation of nature, there is great uncertainty in regard to the factors in the evolution process. How do variations

* *Penycuik Experiments*, 1899.

arise? In what proportion are they continuous or discontinuous, definite or indefinite? How far is natural elimination discriminate? To what extent is isolation demonstrable?—before these and a score of similar questions we stand not less expectant—but perhaps less confident—than the evolutionists of a third of a century ago. It is not that we are where we were thirty years since; it is rather that we have become more aware of our ignorance and of the complexity of the problem.

It is a critical mood that becomes us as a reaction from earlier enthusiasm, and the value of this is borne out by the history of science which shows that the rate of intellectual progress may be measured by the periodicity of the wave of scientific scepticism. But it is not a hands-in-the-pockets scepticism that becomes us as evolutionists, it is a *thätige Skepsis,*—eager to test and measure, to experiment and observe. After half a century of measurement and experiment, the voice of the evolutionist will probably regain confidence. What is especially needed is a national or inter-national institute of experimental evolution where the trials and testings could be continued for generations by a carefully recruited staff, and thus remain unaffected by the death of individual workers.

BOOK FOUR.

PSYCHOLOGY, ANTHROPOLOGY, AND SOCIOLOGY.

(MIND, MAN, AND SOCIETY.)

CHAPTER XII.

PROGRESS OF PSYCHOLOGY.*

PSYCHOLOGY is "the positive science of mental process"; it investigates mental events in their coexistence and sequence, or mental products in their subjective aspect. It has to do with the racial evolution of the mind and the development of the individual consciousness, but not with what ought to be in thought or in conduct (logic and ethics), nor with the nature of knowledge as such (metaphysics).

Its data are obtained from a study of the products of past mental processes and of the stages of processes presently occurring or just fading into the past. Its methods are introspection and retrospection, observation and experiment. And it aims, like other science, at restating the facts in general formulæ, or in

* The aim of this chapter is simply to illustrate four noteworthy changes in the aims and methods of psychology which may be called characteristic of the nineteenth century.

binding them into an intelligible system by interpretative hypotheses.

CHANGES IN AIMS AND METHODS.

Even those who insist that psychology is an ancient science (from Aristotle's *De Anima*) and not one of the newest, will allow that the nineteenth century, especially in its second half, witnessed great changes in the aims and method of psychological enquiry. The advance of physiology made a franker recognition of the correlation of mind and body imperative; a growing intensity in the scientific mood intruded methods of experimentation into a sphere wherein they were formerly conspicuous by their absence; the naturalist advanced a plea for the consideration of the animal mind alongside that of man; and the grip of the evolution-idea made itself felt in the conviction that the "mind" must be studied as the product of individual development and of racial history.

As Prof. E. B. Titchener expresses it:—(1) "Modern psychology works upon the hypothesis that there is no psychosis without neurosis; no sooner has it analysed a mental complex than it begins its search for the neural substrate of the elementary conscious processes." . . . (2) "Experiment has been introduced, not to oust the old-fashioned method of introspection or self-observation, but to control it and standardise its conditions, lifting the 'facts' of psychology from the plane of opinion to the plane of knowledge." . . . (3) Here we would interpolate that psychology has followed physiology in becoming comparative. (4) "Mind, instead of being dissected and classified, in purely logical terms, into static bits

of knowledge (ideas) and empty faculties of knowledge (memory, imagination) is looked upon as an organic structure, that is, as a structure that has grown or developed, to be investigated by analytical and genetic methods." *

"Whether or not we admit the advent of a new psychology, at least we cannot deny the consummation of a great and far-reaching change in psychological aims and methods." †

CORRELATION OF MIND AND BODY.

During the nineteenth century various views were held on this subject.

(*a*) Ignoring what had been clearly shown even by Descartes, and the truth in Hartley's *Observations on Man* (1749) a certain school practically denied that any correlation of mind and body existed. The body and its organs, on one side, the mind and its organs, on the other, were thought of as entirely independent existences. This position is untenable. Certain lesions of the brain are always associated with certain disorders of language, as in aphasia. Conversely, over and over again, the saving skill of the surgeon at the best, or post-mortem examination at the worst, has verified an inference from a particular mental disorder to a disturbance of a particular part of the brain. The general correspondence throughout Vertebrates between the relative size and complexity of the brain and the animal's grade of intelligence, cannot be a coincidence.

Historical Note.—Although at many different

* Summarised from *Recent Advances in Psychology*, *Internat. Monthly*, II. (August, 1900), pp. 154–168.

† E. B. Titchener (1900), *loc. cit.* p. 154.

dates sagacious thinkers had pointed out that the flesh not only wars against the spirit, but in a humiliating way conditions its activity, the recognition of the intimate correlation of body and mind is practically one of the great results of the nineteenth century.

The new doctrine that the brain is the organ of the mind was certainly helped by the industrious work of Franz Joseph Gall (1758–1828) and Johann Gaspar Spurzheim (1776–1832) the founders of phrenology, doubtless an erroneous system, but—like alchemy or astrology—of some service to science. Among the other pioneers were Magendie and Louis Antoine Desmoulins who worked together on the nervous system of Vertebrate animals (1825); Charles Bell who in 1811 discovered the distinction between motor and sensory nerves, afterwards confirmed by Johannes Müller and by Magendie; Marshall Hall, who first elucidated the phenomenon of reflex action (1832); and Flourens who was one of the first to enquire with precision into the functions of different parts of the brain.

In 1825 Boillard, working from the pathological side, had tried in vain to convince his contemporaries as to the existence of an articulation-centre in the frontal lobe of the brain, and there were other pioneers. Little heed was paid to the idea till 1861, when Broca announced his discovery that a definite area in the cerebrum (Broca's centre) was concerned with articulate speech. He thus initiated a more intimate study of brain localisation. Fritsch and Hitzig, Ferrier, Hughlings Jackson, Franck and Pitres, Munk and Goltz, Horsley, Schäfer, Flechsig, Schrader, Steiner, have been prominent workers on this line—endeavouring to map out the brain into

specialised centres both sensory and motor. And to this experimental investigation there has come aid from histological studies, especially since the refinement of methods due to Golgi and Ramón y Cajal.

Although a splendid beginning has been made, it is only a beginning, and even among experts there is much diversity of opinion on important questions. Thus we find Flechsig mapping out three levels of centres in the cortex, *sense-centres* (also motor), *association-centres* (with indirect motor connections), and between these in order of development *intermediate centres;* while, on the other hand, we find Loeb * maintaining that while there exists to a certain extent an anatomical localisation in the cortex, the assumption of a physical localisation is contradicted by the facts. . . . "In processes of association the cerebral hemispheres act as a whole, and not as a mosaic of a number of independent parts. . . . It is just as anthropomorphic to invent special centres of association as it is to invent special centres of coordination." †

SUMMARY.—*It must be admitted by all that "there exist manifold correspondences of the most intimate and exact kind between states and changes of consciousness on the one hand, and states and changes of brain on the other. As respects complexity, intensity, and time-order the concomitance is apparently complete. Mind and brain advance and decline* pari passu; *the stimulants and narcotics that enliven or depress the action of the one tell in like manner upon the other. Local lesions that suspend or destroy, more or less completely, the functions of*

* Loeb, *Comparative Physiology of the Brain* (1900), p. 262.

† Loeb, p. 275.

the centres of sight and speech, for instance, involve an equivalent loss, temporary or permanent, of words and ideas." * *The close parallelism of the two sets of facts is certain; the difficulty is how to conceive of their relation.*

(*b*) With the advance of physiological analysis, a materialistic school found confidence to claim psychology as entirely a branch of physiology. In crude expression, it was said that as the liver secrets bile, so the brain secretes thought; or, that as the collisions in a swarm of meteors engender heat and light, so the whirlpool of molecules within a ganglion has part of its energy expressed as consciousness.

This conclusion includes two distinct assumptions:—(1) that material agency is the only real condition of protoplasmic metabolism (or bodily life), and so likewise of consciousness or mental life, and (2) that physiological interpretations are sufficient for mental occurrences. The first assumption is a metaphysical dream involving the fallacy of "postulating mechanism as the substratum and not as the conceptional expression of certain groups of sense-impressions" (Pearson); the second assumption has not been justified by *any* success. No one has succeeded in giving a physiological interpretation of any mental process; though the physical conditions attendant on many mental processes are known, the relations between the two have not been apprehended.

A quotation from Dr. G. F. Stout's *Analytic Psychology* (1896) may be permitted here:—

"Those who deny agency to consciousness, finding that mental events occur which are not immediately

* Prof. James Ward, *Naturalism and Agnosticism*, 1899, Vol. I. p. 10.

traceable to other mental events, assume that they are due to material agency. Similarly those confronted by material changes not easily traceable to mechanical antecedents, have often assumed that they are due to spiritual agency. How can the modern materialist show that he has any better guarantee for his position than the untutored Indian has for his? . . . If the continuity of the mechanical process debars us from regarding a movement as due to a volition, it must in like manner debar us from regarding a volition as due to movement, even of brain particles. . . . No analysis can discover in the psychological fact any traces of its supposed physical factors." *

(*c*) As physiology has become more modest in realising its own limits of interpretation, and as the psychologist has without mistrust sought to avail himself of all the help the physiologist can give, a more reasonable position has been attained. "Psychology is distinguished from the physical sciences inasmuch as their aim is to know the material world, whereas it deals with the question how this knowledge arises." † "Mental processes cannot be explained as special complications of processes which are not mental, nor can they enter into the composition of such processes." ‡ "No consideration of the physical antecedents as such needs to be included in any strictly psychological proposition. We take account of them only in so far as they are indispensable helps in determining and defining the nature and order of changes produced in the mind from without. The psychologist is primarily concerned not with the antecedents of externally initiated changes, but with these changes themselves, inasmuch as they modify

* Stout, pp. 5–6.

† G. F. Stout, *Analytic Psychology*, Vol. I., 1896, p. 8.

‡ *Op. cit.*, p. 6.

preceding and determine succeeding mental states. Thus, though these physical facts supply data indispensable to the solution of psychological problems, yet they do not themselves belong to the essential subject-matter of psychology." *

But the position of this acute thinker might be misunderstood if we did not quote further. "The life of the brain is part of the life of the organism as a whole, and inasmuch as consciousness is the correlate of brain-process, it is conditioned by organic process in general. It is clear that the unity and connection of psychical states cannot be clearly conceived without taking into account the unity and connection of the processes of the organism as a whole." †

No enthusiast for physiological interpretation could at present wish for a more friendly greeting.

But what of the future, since physiology is advancing by leaps and bounds? "Let us consider what would happen under ideally perfect conditions. If the physiologist were to attain to as clear and definite a conception of brain processes as the physicist possesses of light and sound vibrations; if he had also an acquaintance with psychology sufficient to enable him to set about establishing definite connections between elementary mental and elementary physiological occurrences; if, finally, he had at his command psycho-physical means and methods adequate to this undertaking—then, indeed, we might hope for abundant and valuable results. Indeed, it would seem that under such conditions psychology would be wholly absorbed into physiology so that a single indivisible science would result. But at present we appear to be as far from such a consummation as

* Stout, p. 27.

† Stout, p. 28.

from the establishment of a penny-post between the planets of the solar system." * This is one of the finest specimens of ironical scientific literature since science began.

When the question is asked in this form:—Do the formulæ of biology, of physiology, of chemistry and physics, suffice to restate the facts of mental life? there is at present no manner of doubt that the answer should be an emphatic "No."

Whether the development (personal) and evolution (racial) of that synthesis which we call *Mind* ("the unity of manifold successive and simultaneous modes of consciousness in an individual whole") can be traced is another question, to which the sanguine would—with some justification—answer "Yes."

Whether we shall ever be able to conceive how it is that protoplasmic metabolism comes to be in certain cases attended by consciousness (which we cannot positively define) is another question, answers to which are mere matters of opinion. The correlation and parallelism of metabolism and mentality, of neuroses and psychoses must be admitted, but the two sets of facts cannot be identified, and science as such has at present no answer to give in regard to the nature of the relation between them. We may simply state the three *metaphysical* alternatives:—(*a*) that the brain is the only real agency and consciousness one of its phenomena; (*b*) that consciousness is the reality of which the correlated brain-process is a phenomenon; or (*c*) that brain-process and consciousness are two aspects of the same reality.

SUMMARY.—*The physiologist who devotes himself to the study of nervous functions often speaks as if*

* Stout, *loc. cit.*

his science was in process of absorbing psychology, or rather of showing that psychology is illusory, for he will replace such metaphysical conceptions as soul, consciousness, and will by " real *physiological processes" (Loeb). He has not yet succeeded in this process of substitution, and it appears to us that his expectation or his mode of stating it reveals a misunderstanding.*

At the same time, this anti-metaphysical physiology, of which Professor Ernst Mach * of Vienna is an outstanding champion, expresses a true ideal *for physiology.* For there the terms of interpretation ought to be entirely objective (i.e., as objective as any general terms like stimuli, neuron, neuroses, can be), and terms like consciousness and will are irrelevant.

EXPERIMENTAL PSYCHOLOGY.

The introduction of experimental methods into psychological research was one of the distinctive steps of the nineteenth century, but as most of the results have been gained since 1878 when Wundt opened his laboratory of physiological psychology at Leipzig, it is still too soon to estimate their value. Although Wundt has been the direct inspirer of most of the modern work—whether in opposition or in agreement—we may go further back to Johannes Müller and Weber, to Fechner and Helmholtz.

Johannes Müller (1801–1858).—To this genius we owe the discovery of the law of the "specific energy of the senses,"—that the same stimulus, the same external phenomenon, acting on different

* E. Mach, *Contributions to the Analysis of the Sensations*, trans. Chicago, 1897.

organs of sense always produces different sensations; and that different stimuli acting on the same organ of sense always produce the same sensation. Bunge, from whom we have quoted the statement of the law, calls it "the greatest achievement both of physiology and psychology," "the greatest and deepest truth ever thought out by the human intellect." * "There is," Verworn † says, "scarcely any physiological discovery which has a more important bearing upon all psychology and the theory of knowledge—although unfortunately it is not generally appreciated—than the doctrine of the specific energy of the nerves or organs of the special senses." The doctrine implies "that the external world is not in reality what it appears to us to be when perceived through the spectacles of our sense-organs; and that by the path of our sense-organs we cannot arrive at an adequate knowledge of the world."

We have already noted that Müller was mistaken in referring to the *specific* effects of stimulation to the *nerves*, for since the work of Vulpian (1866) it has been recognised that nerves are simply conducting threads; the specific functions had to be shifted to the cells of the nerve-centres. Moreover, Dr. Hill ‡ refers to the remarkable experiment by which "the vagus nerve, which ought to be supervising digestion and the beating of the heart" can be made "to control blushing, dilation of the pupil, and the other actions which were formerly (are normally) within the province of the cervical sympathetic. This up-

* G. Bunge, *Text-Book of Physiological and Pathological Chemistry*, trans. 1890, p. 12.

† M. Verworn, *General Physiology*, trans. 1899, p. 21.

‡ *An Introduction to Science*, 1900, p. 125.

sets our notions of the specific functions of nerve-*centres.*"

There is reason to suspect that Müller's law, while expressing an important truth, has inclined many physiologists to put in a full stop prematurely. Let us notice how Loeb regards it; his revolutionary or evolutionary outlook is always stimulating.

"Whether a blow, an electric current, or ether-vibrations of about 0.0008–0.0004 millimetres wave length stimulate the retina, the sensation is always a specific one, namely, light, while a blow or an electric current produces sensations of sound in the ear. This so-called law of the specific energy of the sense-organs is not peculiar to the sense-organs; it applies, as was emphasised by Sachs, to all living matter; it even holds good for machines. It is in reality only another expression for the fact that the eye, the ear, and every living organ are able to convert energy in but one definite form—that is, that they are special machines. The determination of the way in which this transformation of energy occurs in the various organs would be the explanation of the specific energy of the various senses."

"Physiology gives us no answer to the latter question. The idea of specific energy has always been regarded as the terminus for the investigation of the sense-organs. All the more credit is due Mach and Hering for first having advanced beyond that limit with their chemical theory of colour sensations. Mach has recently expressed the opinion that chemical conditions lie at the foundation of sensations in general." *

E. H. Weber (1795–1878).—Weber was one of those who introduced precise physical methods into physiological investigation. He belongs to the

* *Comparative Physiology of the Brain and Comparative Psychology*, 1901, pp. 290–291.

school whose illustrious roll includes the names of Volkmann, Ludwig, Helmholtz, E. du Bois-Reymond, and Marey; and he deserves a place in this psychological chapter for his formulation of a law which perpetuates his name and has had a far-reaching influence. It was one of the initiatives in psychophysics.

What Weber tried to find out was the relation between the intensity of sense-stimulus (readily measured objectively) and the intensity of the associated sensation. He found that the degree of keenness in our discrimination between two sensations of weight, light, or sound, varies in constant rates with the total magnitude of the stimuli.

The generalisation may be thus expressed:—"There will be the same sensible difference of intensity between two sensations, provided the relative intensities of the stimuli producing them remain the same. Thus an increase of 1 to a stimulus whose strength is expressed by 100 will be experienced as of the same intensity as an increase of 2 to a stimulus whose strength is 200, or of 3 to a stimulus whose strength is 300, etc. The literature of psychophysics is occupied with the experimental verification, the mathematical development, and the interpretation of this law. But neither its experimental basis nor its interpretation is quite satisfactory." * Its experimental verification is only approximate, especially in regard to light and sound, and there is abundant room for difference of opinion as to its psychological importance. There is a critical summary in Professor Sorley's article from which our quotation is taken.

* Prof. W. R. Sorley, article, *Psychology*, *Chambers's Encyclopædia*.

The history of psycho-physics should give prominence to Gustav Fechner who invented (1860) the term (*Psychophysik*) and first spoke of "physiological psychology," who was also mainly concerned with a vindication and elaboration of "Weber's Law" (as he called it); and to Helmholtz, who measured the velocity of nerve-messages (1851), supplied a provisional physiological basis for the interpretation of visual and auditory sensations, and stood firm by Müller's conclusion that our senses afford us only symbols of the outer world. Mention should also be made of two general works which had a strong influence: Hermann Lotze's *Medicinische Psychologie, oder Physiologie der Seele* (1852) and Herbert Spencer's *Principles of Psychology* (1855). During the last twenty-five years the most prominent figure in Psycho-physics has been Wilhelm Wundt. Among those who have followed him or have struck out on independent lines we may note:—Baldwin; Bethe; Ebbinghaus; James; Pierre Janet; Kraepelin; Ladd; Lange; Lipps; Loeb; Lloyd Morgan; Münsterberg; Ribot; Titchener.

The utility of the experimental method is (1) in giving point and precision to introspection, (2) in making a certain amount of measurement possible, and (3) in correlating definite variations in mental process with definite variations in the conditions.

COMPARATIVE PSYCHOLOGY.

A new day began in Physiology when Johannes Müller made it a comparative study; and although the study of the animal mind has not, as yet, yielded such rich results to the psychology of man as might

perhaps have been expected, an auspicious beginning has been made.

Historical Outline.—Though Descartes set a splendid example, there were few in pre-Darwinian days who even attempted a scientific study of the animal mind. Even those who were careful observers usually remained content with theological or metaphysical interpretations. H. S. Reimarus, who published a large work on Instincts in 1760, and the philosopher Schelling may be named as representative.

The development of physiology (e.g., the theory of reflexes) and of human psychology, and the influence of the evolution-idea, led to a more scientific outlook. Alfred Russel Wallace and others showed that many cases of alleged instinctive activity were really cases of rapid learning and that "instincts" were neither so perfect, unerring, or stereotyped as had been supposed. An attempt was made to arrange vital activities in a psychological series—as if on an inclined plane—automatic physiological rhythms, simple reflexes, complex reflexes, instinctive activities, habitual intelligent actions, intelligent behaviour, and rational conduct. Theories as to the origin of instincts began to abound, the Lamarckian school regarding them as the outcrop of inherited habits (either intelligent activities or complex reflexes to start with), the strict Darwinian school regarding them as the result of the action of Natural Selection on congenital cerebral variations.

Although the term "instinctive activity" is still used to include several different modes of action, we have placed it on the inclined plane between reflex action and habitual intelligent action. Instinctive activities differ from habitual-intelligent activities in

beng inborn or innate, requiring a liberating stimulus, but neither experience nor education, though they are often perfected thereby. They seem to be shared by all the members of the species in almost the same degree, though those of the male may differ from those of the female, and they are of critical moment in the struggle for existence. They differ from simple reflexes in involving the activity of the higher nerve-centres, and there seems no sufficient reason for denying that they may be accompanied by some measure of consciousness.

Among the many contributions to the study of instincts, we recall those of Bethe, Büchner, Darwin, Forel, Groos, G. H. Lewes, Wesley Mills, Lloyd Morgan, J. J. Murphy, Romanes, Schneider, Spalding, Spencer, Thorndike, Vogt, A. R. Wallace, Wasmann, Weismann, C. O. Whitman, Ziegler.

Although the progress of research has already made many of his conclusions more than doubtful, George John Romanes (1848–1894) should, in our opinion, be remembered as one who did much to place the study of comparative psychology on a scientific basis. In his *Animal Intelligence* (1881) he tried to sift the wheat of facts from the chaff of anecdotes; in his *Mental Evolution in Animals* he distinguished *primary* instincts, which arise, apart from intelligence, in the course of natural selection, and *secondary* instincts, which arise by the habituation and inheritance of originally intelligent behaviour; in the same volume and in his *Mental Evolution in Man* (1888) he made a detailed comparison of the mental life of man and of animals.

Some Lines of Modern Work.—An escape from "the muddy quagmire of verbal dispute and the will-o'-the-wisps of irresponsible speculation" is indi-

cated in the beginning of the experimental study of instinct. This is well expressed in the work of Prof. C. Lloyd Morgan, e.g., in his study (following Spalding) of young chicks hatched in an incubator, away therefore from all parental influence.*

Bethe—another careful experimenter—has recently done good service in bringing to a focus the interpretation of the behaviour of ants and bees as that of reflex machines or automata,—a return to the position of Descartes. After intricate meanderings (marked on smoked paper) an ant finds a food-treasure; it returns to the nest and comes back to the spoil with reinforcements; but it is only in the course of many journeys that the circuitous path becomes straightened, as the scent-marked trail is definitised. It seems all "chemo-reflex." A strange ant, dipped in a solution of the pounded ants of another nest, is received by its normal enemies with friendliness. The home-coming bees which usually fly to the doorway of the hive, like arrows to their mark, are quite nonplussed if the hive be shifted a few yards aside. Even if the hive be simply reversed they cluster in futile excitement at the back wall.

In 1889, Verworn published an account of his experiments and observations on Protozoa in which he showed that most of their actions are reflexes, though some show as it were traces of being impulsive.† A different view was maintained by A. Binet ‡ (1891), who convinced himself that unicellular organisms exhibit genuine selective actions. But

* See his *Animal Life and Intelligence* (revised under the title *Animal Behaviour*), also his *Introduction to Comparative Psychology* and *Habit and Instinct*.

† *Psychophysiologische Protistenstudien*, 1889.

‡ *La vie psychique des micro-organismes*, 1891.

Verworn's researches are much more convincing, and have been recently corroborated by H. S. Jennings.*

In his study of the slipper animalcule (*Paramæcium*) and some other Protozoa, Jennings has shown that in all the seeming to seek food or to evade the inimical, there is but one typical motor reaction, like that of a strip of muscle. It may be that a vestige of consciousness persists and that the observable reflex was once represented by a conscious impulsive movement, but the fact seems to be that the slipper animalcule now responds to all sorts of stimuli by one constant kind of movement.

Reference should also be made to the psychological study of some of the outstanding phenomena which occur in the life of many different kinds of animals, e.g., mating (Darwin, Wallace, Büchner, Lloyd Morgan, Groos), or play (Groos). In a most interesting study, Groos seeks to show that play is the outcrop of instincts, evolved like other instincts from congenital variations, and fostered in virtue of their utility. But what can be the utility of play, which by definition has no serious purpose? To which it is answered that play is the young form of work, a rehearsal without responsibilities,—that it lightens the burden of inheritance by affording opportunity for the exercise and perfecting of instinctive activities, and that the play period allows scope for the rise and progress of new variations, initiatives, idiosyncrasies, etc., which form the raw material of progress, before the struggle for existence has become keen.

Open Questions.—We have elsewhere referred to

* "Studies on Reactions to Stimuli in Unicellular Organisms." Numerous papers in *Amer. Journ. Physiol.* and *Amer. Naturalist*, from 1899 onwards.

some of the many open questions* in comparative psychology. Are there *any* cases of animal behaviour which cannot be interpreted without assuming a *conceived*, as contrasted with a *perceived* purpose (reason as contrasted with intelligence)? In what proportion of cases can it be shown that animals utilise their individually acquired experience, adapting their behaviour in reference to what they have learned, or in relation to some quite novel situation? To what extent can we interpret the routine life of an animal, say ant or bee, as a series of reflex actions? How have instincts been evolved?

Nervous Mechanism.—Before we try to make clear the present-day antithesis between the two schools of "comparative psychologists"—those who would interpret all the phenomena in objective physiological terms, and those who maintain that psychical interpretations are equally essential,—we must devote a few paragraphs to stating the generally accepted conclusions in regard to nervous mechanism.

In the simplest animals (Protozoa), there is irritability and conductibility in the protoplasm; there is nervous function, in short; and there are many interesting modes of behaviour, but there is no distinctly nervous structure. Some of the polypes show in simple form the essential ground plan of all the nervous mechanisms of higher animals. A superficial sensitive cell is connected by a fibre with a more internal nerve-cell or ganglion-cell, which gives off a fibre to a muscle-cell. If we multiply each of these component parts a thousand-fold, we have a sense-organ receiving stimuli, a sensory nerve transmitting these, a nerve-centre or ganglion receiving, storing, co-ordinating

* *Science of Life*, p. 207.

and shunting the stimuli, and a motor nerve passing from the ganglion to a muscle.

Up to a certain level in the animal kingdom the behaviour is on the whole very simple, and from a physiological point of view may be summed up in the phrase "reflex action."

"A reflex is a reaction which is caused by an external stimulus, and which results in a co-ordinated movement, the closing of the eyelid, for example, when the conjunctiva is touched by a foreign body, or the narrowing of the pupil under the influence of light. In each of these cases, changes in the sensory nerve-endings are produced which bring about change of condition in the nerves. This change travels to the central nervous system, passes from there to the motor nerves, and terminates in the muscle-fibres, producing there a contraction. This passage from the stimulated part to the central nervous system, and back again to the peripheral muscles, is called a reflex. There has been a growing tendency in physiology to make reflexes the basis of the analysis of the functions of the central nervous system, consequently much importance has been attached to the underlying processes and the necessary mechanism." *

The question to which so much attention has been turned in the closing years of the nineteenth century is as to the proportion of animal behaviour which can be covered by this concept of reflex action. At what level do animals begin to learn, to profit by experience, to adapt their behaviour to novel conditions? Moreover, what security is there in the assumption that the reflex actions which are simplest are also the most primitive? To what extent may they

* J. Loeb, *Comparative Physiology of the Brain*, 1901, pp. 1–2.

be the degenerate descendants of impulsive (or even more complicated) actions?

There can be no doubt that a healthy intact frog or newt controls and selects some of its modes of activity, while it is, to say the least, very difficult to prove that a jelly-fish does so. Yet the jelly-fish has got complex sense-organs and a well-developed, though not very complex, system of nerve-cells. What is it that makes all the difference between frog and jelly-fish? The answer is given in part by a familiar experiment. "Remove the brain of the frog (an operation which it bears with remarkable impunity), and carefully keep it moist and fed, and for the rest of its life, which may easily be prolonged for a year or eighteen months, we have in our hands a machine which responds infallibly to every stimulus, but never makes a move in the absence of an easily recognised provoking cause." *

But while the above experiment shows that the brain is the seat of control, we require some more precise answer, for the brain has many different parts. And here we are helped by one of the elementary facts of minute anatomy, that while the grey matter (a network of nerve-cells) in the spinal cord and in certain parts of the brain receives sensory nerves and gives origin to motor nerves, the grey matter of the surface or cortex of the brain is in a measure apart, acting and being acted upon through the mediation of the other grey matter in the lower parts of the brain or in the spinal cord. It is then in this cortical grey matter that we look for the seat of that power of choice and control that distinguishes the higher animals.

* Dr. A. Hill, *Trans. Vict. Inst.*, XXVI., 1892–93, p. 38.

Minute anatomy has made it possible to map out many of the possible routes in the spinal cord and brain which was no long time ago an un-mapped country. But it is like a country in which, though the roads are known, no passenger has ever been seen, and where the possibilities of short-cuts across the fields are endless. "One thing is quite certain, namely, that the routes which are most frequently used are the most open, and therefore the most easily traversed." Measurements of the time taken by nervous impulses in travelling from part to part of the body make this clear.

It is usual to call the possible path of a sensory stimulus from, let us say, the finger to the spinal or basal brain ganglia, and of a resulting motor stimulus from the ganglion viâ motor nerve fibres to the muscles, a complete arc. And what we have to conceive of is that part of the impulse may be in many cases diverted from the short arc and ascend to the brain-cortex, there provoking impulses which descending fibres carry back to the short arc. It is in some such way that reflex actions may be strengthened or restrained by the control of the higher nerve centres.

The familiar "knee-jerk" is a good example of a pure reflex, occurring in sleep, in the hypnotic state, in unconsciousness,—not much of an action, indeed, but enough to link us back physiologically to the jelly-fish with its pulsating disc. From this simple reflex, with consciousness at zero as far as it is concerned, we can make a long inclined plane on which are arranged more complex reflexes, compound reflexes, reflexes which are apt to arouse consciousness, and reflexes which are very liable to be influenced by conscious control.

"In its first origin the nervous system is like an open moor, equally easy and equally difficult of passage in all directions, but the nervous system as we inherit it is a labyrinth of paths." Some of these paths are trodden down in antenatal life, but of many of them we can only say that their making is part of our inheritance. But here, as elsewhere, the question of origins cannot at present be answered with any confidence.

Animal Behaviour.—Let us take a broad survey of animal behaviour. All around us, except in our cities, there is a busy animal life, swayed by the twin impulses of "Hunger" and "Love." There is eager endeavour after individual well-being, there is not less careful effort which secures the welfare of the young. The former varies from a keen and literal struggle for subsistence to a gay pursuit of æsthetic luxuries; the latter rises from physiologically necessary life-losing and instinctive parental industry to remarkable heights of *what seem to us like* deliberate sacrifice and affectionate devotion.

On the one hand, we see *struggle,* between mates, between rival suitors, between nearly related fellows, between foes of entirely diverse nature, between the powers of life and the merciless forces of the inorganic world. On the other hand, we see the love of mates, family affection, mutual aid among kindred, many quaint partnerships and strange friendships and infinite inter-relations implying at least some measure of mutual yielding.

We watch the wondrous industry of birds and bees who work from the dawn until the dusk brings enforced rest to their brains, which we know to suffer fatigue as ours do; on the other hand we see the

parasite's drifting life of ease. Here locust eats locust, and rat rat; there in the combat of stags lover fights with lover till death conquers both; and again we see a mother animal losing her life in seeking to save her children. At one pole we see simple brainless creatures pursuing their daily life in what we can hardly call more than dull sentience; again we marvel at an instinctive skill whose expression is unconscious art; finally we are face to face with an intelligent behaviour which seems at once a caricature and prototype of our own.

When we talk to naturalists or read a number of works on natural history, we soon recognise that there are two extreme positions. One of these has been briefly described in the phrase "The man in the beast." It is that which interprets an animal's action forthwith as if it were human, which credits the beast with the man's qualities of feeling and reasoning without seeking to prove their presence, which, in short, reads the man into the beast. Now this is generous, and the interpretation of animal life which results is pleasing, and free from the usual self-conceit of human intelligence. Most children pass through it, some naturalists die peacefully in the faith of it. But if comparative psychology has taught us anything, it is that this position is fallacious. He is still at the feet of Uncle Remus, who credits animals with his own qualities without proving his pleasant poetry.

The other extreme is that of those who erect between themselves and the beast a high wall. At no price will they let the man into the beast, nor admit the man in the beast. They are far from agreeing with Scheitlin, the author of a *Versuch einer vollständigen Thierseelenkunde* (1840), who said,

"Nicht aller Mensch ist im Thier, aber alles Thier ist im Menschen." The construction of this high wall between man and beast varies considerably. It is not of course without the hard stones of fact, but is usually cemented with superstition. Those who build it seldom look over it, not that they do not exalt themselves, but they suffer from timidity or from lack of the curious spirit.

If they happen to observe how like to human conduct the behaviour of animals often is, the resemblance is hastily explained away as a mere analogy. In comparing human conduct with that of animals, we must, we are told, ever remember that it is a person, a soul, a *homo sapiens*, a man who acts. Sometimes the distinction is confessedly apparent; at other times we wish we could forget it.

Sometimes the height of the separating wall is made to depend not so much on "the unique majesty of human nature" as on the "marked inferiority of the brute." The animal is seen as an eft in the moat around the human citadel. It is said to have no soul, no intelligence, no control, even no consciousness.

Such then, sufficiently outlined for our purpose, are the two extreme views, that which reads the man into the beast, and that which rears an unsurmountable wall between them, that which makes of an individual *Lepus cuniculus* frisking on the links a Brer-rabbit, or that which regards him as a whimsical automatic machine.

A Compromise.—Between the two extreme interpretations indicated above it seems necessary to find a compromise. We are sure of a conscious mental life in ourselves,—it is our greatest certainty; we infer it in other people,—without this postulate there

could have been no science at all; we usually admit its existence in the higher animals, like birds and mammals, partly because it seems the simplest postulate that will cover the facts, and partly from our general acceptance of the idea of evolution; but as we descend to ants and bees, earthworms and jellyfishes, the impression of automatism grows upon us, we are without any criterion that will enable us to decide as to the presence or absence of conscious control or intelligence or the like, and in particular cases it is often a matter of opinion whether the behaviour of the animal requires psychical terms at all for its re-description.

If we adhere to the law of parcimony, we must seek to interpret as reflexes as much of animal behaviour as will bear this interpretation, but no amount of success in so doing can prove the absence of consciousness. Furthermore, when we reflect that it often requires close acquaintance to discover intelligence in the behaviour of our fellow-men,—whose actions are often complex reflexes or automatic—we are induced to be cautious in our inferences as to animals. Especially with subjects like ants and bees, we feel the difficulty of getting sufficiently near them to detect the individual peculiarities of behaviour in which intelligence may reveal itself.

Our opinion at present is that since a number of lower animals give evidence of memory for localities, for sounds, for particular kinds of food, etc.; since others show some power of profiting by experience, or of educability; since others seem able to depart from the usual responses of their reflexes when novel circumstances demand a departure from routine, and so on, we cannot give even a descriptive account of their behaviour without introducing psy-

chical terms, such as intelligence and conscious control. And this position is strengthened by the fact that we find structural nervous complications, in a gradually ascending series, comparable to those which we know to be the physical basis of mentality in ourselves. We need not be so generous as the earlier observers who made each animal a homunculus; but we cannot pretend to be convinced that the progress of physiology has yet justified us in accepting the phrase "reflex-machine" as an adequate description of even a pismire.

Father Wasmann,* who has done splendid work as an entomologist, especially in connection with the partners and guests of ants, has recently sought to uphold the view that many animals must be regarded as actively intelligent, or with psychical life which, within its acknowledged limits, is as essential to their behaviour as ours is to our daily conduct. In other words, he has argued against the purely objective interpretation of animals as "reflex-machines." In referring to this Professor Loeb notes that the answer to the question whether or not animals possess intelligence varies with the definition of the word, and that the discussion is purely scholastic. "The aim of modern biology is no longer word-discussion, but the control of life-phenomena. Accordingly we do not raise and discuss the question as to whether animals possess intelligence, but we consider it our aim to work out the dynamics of the processes of association, and find out the physical and chemical conditions which determine the variation in the capacity of memory in the various organisms." † And

* *Instinct und Intelligenz im Thierreich*, 1897.

† *Comparative Physiology of the Brain and Comparative Psychology*, 1901, p. 287.

he looks for the interpretation of memory in terms of the nature of the colloidal substances which make up protoplasm.

This seems to us an admirable position for the *physiologist,* to whom subjective terms are irrelevant, but "comparative psychology" is part of the title of Loeb's book, and therefore we doubt if the author is justified in calling the question of presence or absence of intelligence a scholastic discussion.

Our point is simply this, that while the purely physiological interpretation may seem sufficient (we are only half-convinced) to account for certain events in the behaviour of sea-anemones, jelly-fishes, worms, etc., as most graphically depicted by Loeb, it is not as yet even approximately sufficient to account for the general behaviour of the majority of animals. We admit that where no evidence of even associative memory can be found, it is difficult to show (except on general grounds) why the hypothesis of psychoses as well as neuroses is necessary. But when we take a broad view of the behaviour of animals, we find the psychological interpretation necessary.

If it be shown that not only the bee but the bird can be adequately described physiologically, that the hypothesis of crediting either with a mental life is gratuitous, that comparative psychology, in short, has disappeared as comparative physiology has advanced, then the number of scientific formulæ has been reduced by one,—that is all. But, in the meantime, this reduction not having been achieved, we are in the habit of studying the behaviour of bees and birds, and must have a theoretical linkage for our facts. We find no other linkage available except

the psychological one, since that afforded by physiology seems to us inadequate to fit the facts.

Another View.—As we wish that our historical balance-sheet, necessarily condensed, should be at least fair, we may direct the reader's attention to the work of Prof. Loeb (already cited as an instance of the purely physiological position). According to Loeb, reflexes may occur without a reflex arc, they are not necessarily bound up with the central nervous system or the ganglion-cells; the central nervous system is only a convenient conductor; instincts are bundles of tropisms; neither for spontaneous activity nor for co-ordination are ganglion-cells essential; the only specific function of the brain, or certain parts of it, which Loeb has been able to find, is the activity of associative memory; and this is made possible by peculiarities (still quite obscure) in the nature of the colloidal substances which form the physical basis of life.

DEVELOPMENT AND EVOLUTION OF MIND.

"We may define psychology," says Dr. G. F. Stout, "as the science of the development of mind." * The definition indicates the modern outlook of the science, but the problems involved are so difficult that we have restricted ourselves to pointing out the various sources of information.

The Data.—From four sets of facts the psychologist may obtain development and material for his conclusions as to the individual and racial evolution of mind.

* *Analytic Psychology*, Vol. I., 1896, p. 9.

(*a*) He may utilise past mental products,—the words and structure of language in which thought is embodied, the beliefs and customs of races, their works of art, and so on.

(*b*) Valuable data are also obtainable by the study of children,—a line of investigation practically begun by Preyer, and at present well represented by Prof. Mark Baldwin * and Stanley Hall.

(*c*) From experimental work—in which the stages of a mental product can sometimes be detected; and from comparisons of normal subjects with the blind or the deaf, another set of data are obtainable.

(*d*) Lastly, some help has been forthcoming from the studies of those who, like Romanes and Lloyd Morgan, have paid particular attention to the animal mind.

CONCLUSION.

We have, in this chapter, briefly illustrated four steps of recent progress in psychology:—(*a*) the fuller recognition of the correlations between body and mind, (*b*) the rapidly increasing habit of resorting to experiment, (*c*) the broadening of the science on comparative lines, and (*d*) the endeavour to look at all the facts from a genealogical or evolutionary standpoint.

We are reminded that there are other important steps,—the beginning of a social psychology (Tarde, Baldwin, Royce, Le Bon); the beginning of a careful psychology of sex (Havelock Ellis); the development of practical psychology in reference to education (James, Lloyd Morgan, and many others); the

* *Mental Development in the Child and the Race*, 2 vols.

application of psycho-physical methods to the study of the abnormal mind; and so on. For here, as elsewhere, we can only illustrate the scientific progress of the century by the unsatisfactory method of sampling.

CHAPTER XIII.

Advance of Anthropology.*

The Subject.—Anthropology has mankind for its subject, just as ornithology deals with bird-kind and entomology with insect-kind. It is, from one point of view, a specialised department of zoology, dealing with one particular species—Man, and it applies zoological methods to the study of human variations and modifications, and to the interpretation of the characteristic features in structure, habit, and social organisation which distinguish the different human races. It is, from another point of view, concerned with what may be called the prolegomena to the scientific study of history, for through linguistics, folk-lore, and the study of the ancient (often prehistoric) remains of human activity it passes gradually into the historical discipline, in the narrower and stricter sense, which takes to do with the period of which we have intentional records.

Anthropology is, like geography, a synthesis or combination of contributions from a number of sciences towards the interpretation of a particular problem—the human species as such. "We must be prepared to take anthropology more as the study of man in relation to various and often independent

* The aim of this chapter is simply to indicate six of the most important problems which have engaged the attention of anthropologists during the nineteenth century.

subjects than as an organic and self-contained science." *

Anthropology has its physical side, based on anatomy and physiology; it has also its psychical side, based (theoretically) on psychology; it has also its social aspect, and leads gradually on to the incipient science of inductive sociology which concentrates its attention on the various forms of social organisation and on their correlation with particular conditions of existence. In the study of skulls, etc., anthropology meets anatomy; but in the study of interesting problems like that of a primitive matriarchate (or maternal group) and its possible relations to the recognition of the family-tie and tribal development, it obviously joins hands with sociology. It is easy enough to confine anthropology by a definition to the study of individual bodily characters and to make ethnology the science of the races of men, but the distinction is untenable, since man is characteristically social.

Impulses.—There were at least three impulses which prompted the noteworthy advance of anthropology in the second half of the nineteenth century. (1) In many ways travelling had become easier, distant parts of the earth became practically near at hand, and materials which were formerly scanty and uncertain became abundant and secure. (2) The increase of colonisation and the expanding exploitation of the earth brought men into familiar touch with races whose names were unknown to their fathers, and anthropology came to have great practical as well as theoretical interest. (3) The influence of Darwin's work was especially momentous,

* Prof. W. M. Flinders-Petrie, Address Anthropol. Section, *Rep. Brit. Ass.*, 1885. p. 816.

for he showed the value of discussing man from a natural history point of view, and shed the light of the evolution-idea on a mass of anthropological facts which had previously been little more than curiosities.

Associated with these there is now another sad impulse, that certain races are in process of rapid elimination; their scientific lesson must be read now or never. An anthropological expedition is urgently needed to study fleeting customs, as E. H. Man and M. V. Portman did for the natives of the Andaman Islands, as Prof. A. C. Haddon did at the Torres Straits, as Profs. Baldwin Spencer and Gillen have been doing in Australia, as Government officials and others are doing for the American aboriginal population.

Perhaps another impulse to careful anthropological study has come from the insistent importance of criminology. The great practical interest of this enquiry has reacted on the science of anthropology from which it had its origin.

MAN'S PLACE IN NATURE.

We use this time-honoured phrase to designate the problem—still far from solution—of man's genetic relationship to some pre-human or Simian stock. Even Sir Richard Owen, conservative as he was, recognised the "all-pervading similitude of structure" between man and the apes, and since Darwin's *Descent of Man* and Huxley's essay on *Man's Place in Nature,* it has seemed quite fair to reject any interpretation which denies man's structural resemblance to some Simian or ape-like type. So far as bodily structure is concerned, Man is plainly one of the Primates. As regards the psychical charac-

teristics of man,—language, reason, morality,—every fair-minded enquirer must admit that it is difficult to disclose the factors which evoked them, but that is hardly an argument against deciding that their mode of origin was evolutionary.

Although the structural resemblances between man and the anthropoid apes are numerous and plain, no one now dreams of arguing that man is descended from any existing form. Different living forms approach man in different ways. At what point the human stock diverged from the Simian remains quite obscure; no certain intermediate links are as yet known,—though some of the oldest known human skulls are primitive in some of their features.

Nor can it be ignored that, as regards various structural characters, some experts have found it necessary to look for man's ancestry even deeper than in the monkey race,—down to the Prosimiæ or Lemuroids.*

Dr. E. Dubois' discovery of remains at Trinil in Java (which he calls *Pithecanthropus erectus*) is interesting and valuable, but they are fragmentary (skull-cap and femur), and experts differ greatly in their interpretation of them. The Trinil femur seems to have been that of a being who stood upright; the capacity of the skull (inferred from the cap) was greater than that of any known anthropoid ape, but inferior to that of human skulls of low type belonging to the Stone Age. The remains are either those of a missing link or of a low and ancient type of man.

"The antiquity of the human race is much greater than was previously supposed; we must go back to the

* Prof. H. Klaatsch, *Globus*, LXXVI., 1899.

Early Tertiary, and to the roots of the Primate stock to find the origin of the species Homo. A precise investigation of the whole Primate-group, of its extinct as well as of its extant members, forms the only basis on which a scientific physical anthropology can be established. Without this comparative anatomical foundation, all theories as to the origin of the human race remain, in my opinion, wholly in the air." *

Apart from mental development, the distinctively human characters are thus summarised by Sir William Turner:—"the capability of erecting the trunk, the power of extending and fixing the hip and knee joints when standing, the stability of the foot, the range and variety of movement of the joints of the upper limb, the balancing of the head on the summit of the spine, the mass and weight of the brain, and the perfection of its internal mechanism." †

But, as is well known, the great gap between man and other living creatures is in mental life, some indication of which is given by man's superiority in brain-development. A man *may* have a brain three times as heavy as a gorilla's; the average human brain weighs 48–49 ounces, the heaviest gorilla brain does not exceed 20 ounces. The figures for volume or cranial capacity are not less striking. (See Keane's *Ethnology*, p. 40.) But these figures will be seen in an altogether false light unless we compare them with the differences between the various kinds of monkeys. The marmoset is farther below the gorilla than man is above it. It is also necessary to take into account the enormous variations that occur within the hu-

* Prof. Rudolf Martin, *Anthropologie als Wissenschaft und Lehrfach*. Jena, 1901, p. 23.

† *Rep. Brit. Ass.*, 1897, p. 788.

man species. Similarly, as to characters which cannot be measured or weighed, it is obvious that it is the mind of a Fuegian and not that of a Newton which should be compared with that of the higher animals.

Although anthropologists are not in a position at present to do more than speculate in regard to the factors which may account for the evolution of man's big brain, the great majority are unhesitating in their acceptance of the general conclusion of Darwin's Descent of Man, *that man arose from an ancestral stock common to him and to the higher apes.*

ANTIQUITY OF MAN.

"Man's immense antiquity is now accepted by a vast majority of the most thoughtful men." * The word "immense" is suitable, for it remains impossible to arrive at incontrovertible data by which to measure the prolonged period which has certainly elapsed since the human race began. We have already referred to the uncertainty which besets any estimate of the age of the earth, and similar remarks apply to the case of man. There are traces of man, or of some immediate precursor in the New or Late Pliocene deposits, along with remains of the mammoth, the woolly rhinoceros, the cave-lion, the cave-bear, the Irish elk, and other extinct mammals once wide-spread throughout Europe and Britain. That man appeared *before the last* of the Pleistocene ice-ages seems undeniable, and it is possible that he had appeared before the first of them. "The most rational hypothesis," Mr. Keane says, "seems that of

* Dr. Robert Munro, Address Anthropological Sec., *Rep. Brit. Ass.*, 1893.

inter-glacial Hominidæ specialised not less, probably much more, than half a million years ago." * Giglioli may be named as another expert anthropologist who regards man's origin as *inter-glacial.* For our present purpose, the long and weary discussions on this subject are of little moment, for though there may be doubts whether a million or half a million or a quarter of a million of years should be claimed, the general tendency among those who know most about it is towards the larger figures, and while, on the other hand, man is but a child of yesterday when the age of the earth is considered.

Let us recall the great periods in man's unwritten history.

(*a*) Since man is certainly not derivable from any of the known anthropoid apes, and since it is likely that he sprang from an ancestral stock common to them and to him, we seem almost bound to conclude that the divergence which led on to the *human* line of evolution must have occurred before the appearance of the *anthropoid* family. But the anthropoids (e.g., *Pliopithecus, Dryopithecus*) were in existence in Miocene times, and the inference is that man's direct precursors had also appeared.

(*b*) Before man became habitually a user of tools and weapons, there probably was a long period when he used such sticks and stones as came readily to hand. Even monkeys occasionally do so. Although we do not know with security of any implements older than palæolithic axes and hammers and the like, it is plain that the making of these implied no small skill and a previous period of apprenticeship.

(*c*) The data for the study of the prehistoric evo-

* *Ethnology*, 1896, p. 69.

lution of man are derived from his bones, from his implements, and from the remains of his homes and monuments. To Sir John Lubbock is due the now universally used term "palæolithic" for the first of the prehistoric periods with definite data, and the second half of the nineteenth century is rich in researches on this ancient era. It is probable that palæolithic man (defined by remains in the interglacial epoch) had already spread over nearly the whole world, that he knew naturally-kindled fire, that his diet, at first mainly vegetarian, became more carnivorous as hunting and fishing developed, that he had no cultivated plants, no houses, no monuments, that he made stone implements but did not grind or polish them, that he made a few personal ornaments, that he could sew, and that he sometimes drew with considerable skill. In this period the state of man is often described as "savage." See A. H. Keane's *Ethnology* (1896), p. 110, and Tylor's *Anthropology* (1881).

(*d*) In Neolithic times, man seems to have been able to make fire and to have sometimes cooked his food; to hunting and fishing he had added stock-breeding and tillage; there were many cultivated plants; he had houses, barrows, graves, and monuments (single blocks or polylithic cells); his industries extended to making polished stone instruments, spinning, weaving, mining, pottery-making, carpentry, and boat-building. In this period the state of man is often described as "*barbaric*." Between the palæolithic and the neolithic periods, there often seems a hiatus (as in Britain), but there is evidence elsewhere (in southern and south-eastern lands) of continuous evolution.

(*e*) After the neolithic ages, but still prehistoric, come the metal ages,—the copper age ("crowded out almost everywhere in the Old World"), the bronze age, and the iron age (the two last sometimes coalescing).

Even a moderate estimate would grant 10,000 years to the historical period in Egypt and Mesopotamia, 20,000 to the metal ages, 70,000 to the neolithic period, and behind that total of 100,000 years (since the close of the last ice age) there stretches the vista of the palæolithic, and even then man had a long history behind him.

The interest of these figures is merely to suggest that there was plenty of time for much evolution. "Many things may happen in a long time," and the acknowledged difficulty of interpreting human evolution must not be exaggerated by forgetting that he is not so young as he looks.

Although the date of man's origin remains quite uncertain, the work of the nineteenth century has secured this result at least that man is of great antiquity. It is a moderate estimate to suggest half a million years.

THE HUMAN SPECIES.

The literature on the subject of the human species is enormous, and when we seek for the result, it seems preposterously small. Is there one species of man or are there several? It seems for the most part a verbal discussion, depending on the definition of the term species.

The Linnæan conception of species, from which Biology has not even yet quite freed itself, was that

of an assemblage of forms with characters constant to this extent that the species was permanent and discontinuous from other species. But the evolution-idea has changed this, and we regard species as stages in a progressive development whose flux is so slow that the shortness of any man's observational period is *almost* inadequate to detect it. But the flow of glaciers is not negatived by the fact that they cannot be used as means of transport.

It seems fairly certain that had not the enquirer been man himself (with obvious vested interests) there would never have been any discussion as to the unity of the human species. The numerous races are quite comparable to the races of pigeons (all descendants of the wild rock-dove, *Columba livia*), or to the races of cabbages (all derived from the wild kale); they are all, so far as we know, fertile *inter se,* but precise data on this subject are within a comparatively narrow range; they shade off into one another most perplexingly when identification or definition is the object; in a word, they are *varieties*. There are no certain cases comparable to mules among mankind.

It is possible, of course, that some of the remains doubtfully identified as human may be those of a precursor species; it is possible, also, that some form of "isolation," e.g., psychical antipathy, might even now lead to the evolution of a distinct human species non-fertile with the rest of mankind; but, at present, the conclusion seems secure that zoologically considered mankind represents one species.

We have, however, no enthusiasm on the subject, remembering Darwin's verdict:—"It is almost a matter of indifference whether the so-called races of man are thus designated, or ranked as 'species' or

'sub-species,' but the latter term appears the most appropriate."

Whether we should regard the races of mankind as distinct species, or as sub-species, or as varieties, remains a subject of verbal discussion, but the modern evolutionary conception of "species" has robbed the problem of most of the interest it once had. The important thing is that the modern statistical method of taking account of specific characters should be applied to the races of men, that actually occurring variations should be recorded, and that, as far as possible, all non-congenital differences (due to individual modification) and all artificial differences (e. g., politically defined nationality) should be separated from the congenital characters which alone are indicative of genetic affinity.

RACES OF MANKIND.

The difficulty that has been felt in distinguishing human races is parallel to that which is familiar to the zoologist in regard, for instance, to dogs, or to the botanist in regard to willows or brambles.

"All being fertile *inter se*, although possibly in different degrees, and several having early acquired migratory habits, endless new varieties have constantly been formed since remote prehistoric times, both by segmentation of early groups, and by countless fresh combinations of early established varieties. Outward modifying influences must have been brought into play as soon as the first-named groups began to migrate from their original homes, and such influences, intensified by the climatic changes accompanying the advance and retreat of glacial phenomena, would in-

crease in activity according as the primitive tribes spread farther afield. To these influences of the surroundings were soon added the far more potent effects of interminglings seen to be at work already in neolithic times, and thus the development of fresh sub-varieties of all sorts proceeded at an accelerated rate. This process has necessarily continued down to the present time, resulting in ever-increasing confusion of fundamental elements, and blurring of primeval types. Hence it is not surprising that many ethnologists should accept as a truism the statement that 'there are no longer any pure races in the world.'" *

The history of the classification of mankind into races is not very instructive. The complexion, the character of the hair, and the shape of skull have supplied the chief basis, sometimes in combination, but oftener singly. Consideration of language has also been introduced, but it has been perhaps as much a hindrance as a help. Only of recent years has it been possible to utilise mental, as well as bodily, distinctions, and their usefulness depends on the discrimination of the enquirer.

The first serious attempt at classification is said to be F. Bernier's (1672), but that of Linné, a century later, has had more lasting influence. After setting aside *Homo monstruosus* and *Homo ferus,* Linnæus divided *Homo sapiens* into fair-haired, blue-eyed, light-skinned Europeans; yellowish, brown-eyed, black-haired Asiatics; black-haired, beardless, tawny Americans; and black, woolly-haired, flat-nosed Africans. A close approximation to this classification is now used by many experts.

The work of Buffon and Dr. J. C. Prichard

* A. H. Keane, *Ethnology*, 1896, p. 163.

(1785–1846) lies at the foundation of ethnology, but neither indulged in any special classification. Broca, De Quatrefages, Haeckel, Huxley, and many others suggested schemes, none of which has been found altogether satisfactory. The present tendency seems to be to postpone further construction until the criteria of race have been more thoroughly and more critically studied. The practice of anthropometry was greatly increased in exactness by the work of Quetelet, Galton, and others; but there is still need for careful criticism. Thus the zoological distinction between "variations" and "modifications" has to be worked out in regard to racial distinctions; and the occurrence of "convergence" or "homoplastic resemblance" familiar to the biologist, must be carefully looked for.

It seems fairly clear that in regard to physical characters no reliance can be based on one character by itself. Men cannot be classified by skull-characters (especially if the observations be restricted to adults) as crystals by their facets. The diagnostic distinctions of races must rest on a combination of characters. It seems also clear that speech and race are anything but convertible terms, and that similitudes in customs and belief afford no criterion of genetic affinities. They are analogies, not homologies.

Mr. Keane's picture of the chief branchings of the human genealogical tree is briefly as follows:—(I.) The first ramification from the main stock is that of the "*generalised negro*" (*Homo æthiopicus*), branching off in various directions towards Africa, Oceania, and Australia; (II. and III.) after the negro dispersion the main stem throws off a generalised Mongolo-American limb, which pres-

ently breaks into two great divisions (*Homo mongolicus* and *Homo americanus*); (IV.) between the negro and the Mongolo-American boughs the main stem passes upwards, developing a generalised Caucasic type (*Homo caucasicus*), which also at an early date ramifies into three great branches, filling all the intervening central space, overshadowing the negro, overtopping the mongol, and shooting still upwards, one might say, into almost illimitable space. Such is the dominant position of the highest of the Hominidæ, which seems alone destined to a great future, as it is alone heir to a great past. All the works of man worthy of record have, with few or doubtful exceptions, emanated from the large and much convoluted brain of the white *Homo caucasicus*.*

EVOLUTION OF LANGUAGE.

For millions of years the silence of nature was broken only by the "inanimate" voices of wind and wave, of thunder-clap and cataract. There was no voice of life, until this began among insects, and at a much later stage once again among amphibians. The croaking of frogs is effected by a mechanism (of larynx and vocal cords) essentially similar to that of the prima donna's song. Even a brief study of the vocal sounds made by birds and mammals shows that certain sounds are restricted to certain occasions and have a certain meaning. They express particular emotional states, and they often indicate the discovery of danger or of food. In this sense, there is no doubt that the young chick or the dog has a few definite words. That fairly definite in-

* From Keane, p. 226.

formation may be conveyed by one animal to another without words at all seems a legitimate conclusion from studies on the behaviour of ants, while, on the other hand, there is no evidence that any animals, even monkeys, have language (*logos*) in the stricter sense; that is to say, the use of words in expressing judgments.

From his pre-human ancestry man doubtless inherited the structural arrangements which make language possible,—the vocal cords and their nervous connection with a cerebral centre; but it seems extremely improbable that any hint as to human phonetics will be furnished by the most careful study of jabbering monkeys. It seems likely that language in the strict sense was altogether a human product, following in the wake of that marvellous stride in evolution which gave man his big and richly convoluted brain. That speech helps intellectual development (unless overdone) is certain, but there seems more reason to say that man spoke because he thought than that man thought because he was able to speak. And it would be still more correct to say that man became able to speak partly in virtue of higher cerebral organisation and intelligence generally, and partly because he had gained somewhat subtle nervous connections between the brain and the mouth and larynx. There may have been, as there still is, communication of judgments without a single sentence.

We look back in imagination to the early days of our race, and we suppose that then, as in the early days of individual infancy, there was "no language but a cry." We remember also that the physiological "emotional circuit" within our body affects the muscular movements of heart and lungs, of

larynx and bladder. We look back to the first sentence as a subtle mixture of cry and gesture. It may seem that a great gulf is fixed between the first jabbering sentence and the orations of Demosthenes, but the students of phonesis and language have detected many a hint of the bridge. The child remains as a perpetual illustration, whose significance is by no means exhausted.

That there are great difficulties in accounting for the evolution of language must be frankly admitted, but the enquiry is still young. We must remember the importance of sociality; the possible influence of periods of enforced leisure (which seem to have been important in the evolution of bird-song); the excitements of the chase, of the conflict, of the courtship; the imitative instinct and the hints to invention afforded by the many voices of nature which fell upon the ear of primitive man; and many other considerations.

Apart from the problem of its origin and evolution, language is of great interest to anthropologists as an index of mental character in different races and as a possible aid in their classification. We are no longer liable to the error of making it the sole criterion of race, as some of the earlier philologists maintained in their enthusiasm, but the opposite error of rejecting the philologist's assistance must be avoided. Although data are still few, there seems evidence of structural differences in the organs of speech in different races, and there is no doubt as to the value of the old "shibboleth" test which depends on the auditory as well as on the vocal organs. The value of the linguistic test is increased by the remarkable fact that while peoples mix, languages never do (apart from word-borrowing). The

Basques are shown by their speech to be at least partly descended from a pre-Aryan or a non-Aryan race (African Hamite?), and similarly it may be said of Finns and of Magyars that their speech betrayeth them. "Language used with judgment is thus seen to be a great aid to the ethnologist in determining racial affinities and in solving many anthropological difficulties" (Keane). Into the question of the various lines of language-evolution—Agglutinating, Polysynthetic, Inflecting, and Isolating —it is beyond our scope to enter.

On the other hand, we must remember Prof. Sayce's caution: "A common language is not a test of race, it is a test of social contact. . . . While the characteristics of race seem almost indelible, language is as fluctuating and variable as the waves of the sea."

APPRECIATION OF FOLK-LORE.

The advance of anthropology in the nineteenth century has involved a quite new appreciation of folk-lore, and this has brought much gain to the science. What was formerly regarded as the somewhat mysterious romance of young peoples is now part of the anthropologist's data. So much has it been used, indeed, that the taunt has arisen that anthropology is founded on romance. Let us give one familiar illustration, in reference to folk-lore about the fairies.

It seems that there are fairies and fairies. There are divinities associated with rivers and lakes, and there are dead ancestors, but "in far the greater number of cases we seem to have something historical, or, at any rate, something which may be con-

templated as historical. The key to the fairy idea is that there once was a real race of people to whom all kinds of attributes, possible and impossible, have been given in the course of uncounted centuries of story-telling by races endowed with a lively imagination." * From British folk-lore about fairies, Prof. Rhys has constructed a picture of an ancient race in Britain, "small, swarthy mound-dwellers, of an unwarlike disposition, much given to magic and wizardry, and living underground: its attributes have been exaggerated or otherwise distorted in the evolution of the Little People of our fairy tales." †

With the help of folk-lore and linguistics it may thus became possible to trace a probable succession of British peoples—the Little People, the taller Picts who enslaved them, the Celts, and so on.

Though we must not make a dogma of it, there seems much to be said for the generalisation that similar interpretations and similar modes of fanciful expression crop up at similar stages in the intellectual evolution of different races. The wide dissemination of many old stories, like that of Cinderella, suggests this. "If we view them in their wealth of detail, we shall deem it impossible that they could have been disseminated over the world as they are, otherwise than by actual contact of the several peoples with each other. If we view them in their simplicity of idea, we shall be more disposed to think that the mind of man naturally produces the same result in the like circumstances, and that it is not necessary to postulate any communica-

* Prof. John Rhys, Address to Anthropological Section, *Rep. Brit. Ass.* for 1900, p. 885.

† *Op. cit.*, p. 896.

tion between the peoples to account for the identity. It does not surprise us that the same complicated physical operations should be performed by far-distant peoples without any communication with each other. Why should it be more surprising that mental operations, not nearly so complex, should be produced in the same order by different peoples without any such communication? Where communication is proved or probable, it may be accepted as a sufficient explanation; where it is not provable, there is no need that we should assume its existence."* In this connection reference should be made to the researches of Dr. J. G. Frazer, Mr. E. Sidney Hartland and Mr. Gomme.

There is need to be exceedingly careful with the generalisation that children in their fancies and games, speech and ideas, recapitulate stages in the evolution of mankind. Changed conditions and the influences of education tend to modify such recapitulation as there may be. At the same time, this line of enquiry, cautiously followed, has led to valuable results. Thus the antiquity of many child-games is indubitable; they persist unchanged with remarkable conservatism; to some extent they are vestiges of ancient customs. As Lord Bacon said of fables, we may find in the games of children "sacred relics, gentle whispers, and the breath of better times." The works of Mrs. Gomme and Professor Groos may be especially mentioned.

Increasing attention is also being paid to the anthropological value of the decorative arts. In many cases there is a "racial style," as persistent as a physical feature, recognisable through periods of

* E. B. Bradbrook, Address Anthropological Section, *Rep. Brit. Ass.*, 1898, p. 1005.

thousands of years. The works of Dr. Grosse, Dr. H. Balfour, Prof. A. C. Haddon, and Hirn may be particularly referred to.

FACTORS IN THE EVOLUTION OF MAN.

Enquiry into the factors of evolution is still so young that we find little sure foothold before such a difficult problem as the origin and descent of man. The same may be said in regard to the origin and descent of Vertebrates, or of Birds, or of Mammals, in fact all round. But the difficulty seems great in regard to man, because man's mental characteristics raise him so high above the animals. Indeed, the difficulty of accounting for mathematical, musical, artistic, and moral faculties in terms of the evolution-formula led Alfred Russel Wallace—the Nestor of Nineteenth Century biology—to give up the problem, and to conclude that these faculties must have had another mode of origin, for which "we can only find an adequate cause in the unseen universe of Spirit."

It seems premature, however, to make man—or rather one aspect of man—the great exception, and to abandon the scientific problem as insoluble, after a trial of less than half a century.

The difficulty is doubtless exaggerated by ignoring the facts of anthropology, by thinking too much of Plato and Aristotle, Newton and Goethe, and too little of the savage.

The difficulty is also exaggerated unnecessarily by the relative youth of comparative psychology; we are only beginning to be precisely informed in regard to the intellectual development of the higher animals. We readily refer to them our heritage of

evil dispositions and ignoble propensities which cling about the ascending life as the grave-clothes on the resurrected Lazarus—but we are apt to forget our heritage of good,—the wrinkled brain, the quick sense, the interest in kin, and how much more. Such a work as Sutherland's *Evolution of the Moral Instincts* may be cited for its wealth of evidence as to the content of morality in which man's precursors might well have shared, though we do not think that it fairly faces the difficulty of interpreting the origin of the ethical judgment.

Must we simply fall back upon the general evolution-factors which the biologist seeks to test:—*Variation,* sometimes transilient, often definite; *natural selection,* whose subtlety of influence is becoming ever clearer; and *isolation* in its many forms? Or, are there any particular factors, which, though included in the above categories, may be specially relevant to the case of man? Or is there some unknown factor in evolution which will make the whole matter clear?

(*a*) Dr. Robert Munro has emphasised the importance to the evolving man of the erect attitude, which *Pithecanthropus erectus*—whatever he was—seems to have had, which the anthropoid apes (especially the gibbon) have in some degree. It left the hands more free for manipulation, for using a tool or weapon, for feeling round things and appreciating their three dimensions; it reacted on other parts of the body, such as the spinal column and the pelvis, even perhaps on the larynx, as Jaeger suggested. In his address to the Anthropological Section of the British Association in 1893, Dr. Munro directed attention to three propositions:—(1) the mechanical and physical advantages of the erect position, (2)

the consequent differentiation of the limbs into hands and feet, and (3) the casual relation between this and the development of the brain. But what prompted man or his forerunners to abandon arboreal life and stand erect upon the earth remains a riddle.

(*b*) If we grant the primitive man an erect attitude, the habit of using his hands, a big brain, some words at least, some family life, and so on, as far as the anthropoid analogy will in fairness admit, but deny him strength enough to keep his foothold by that virtue, it may seem more than a platitude to say that natural selection would favour the development of wits, and not only wits, but, in the widest sense, "love," which became a new source of strength.

(*c*) The influence of the family was probably an important factor, fostering sympathy and gentleness, prompting talk and division of labour. Even in early days children would educate their parents. As rudimentary forms of family life are exhibited by gorillas, chimpanzees, etc., there is no reason to make any particular difficulty over its human origin. We are certainly not compelled to believe in original promiscuity, though such phases may have occurred. The conclusions of McLennan and Morgan have to be corrected in the light of the criticisms of Westermarck, Hale, and others. And again it must be remembered that pairing for life or for prolonged periods occurs both among mammals and birds.

(*d*) As the prolonged helpless infancy, characteristic of human offspring, tightened the family bond, and helped to evolve gentleness; as related families combined in a rudimentary clan for protection against wild beasts and other rudimentary clans, there

might arise a heightened sociality rich in progressive influence.

(*e*) With the development of tool-using and sentence-making, with the gaining of firmer foothold in nature, with the occasional emergence of the genius, there might arise—in permanent products, in symbols, in traditions—an *external heritage*, which, it appears to us, has been the most potent factor in securing and furthering human progress. For man is relatively a slowly reproducing, slowly varying organism.

We have not expanded these suggestions, for mere may-be's have no place in science, and a further elucidation of the factors in the evolution of man must be one of the tasks of the twentieth century.

CHAPTER XIV.

Suggestions of Sociology.*

SCOPE OF SOCIOLOGY.

Sociology, though still a very young science, is past the stage of being scoffingly dismissed as "a mass of facts about society." It proposes to give a scientific account of social life as a concrete unity, whose constituents have their significance from their relations to the whole. It proposes to do this by analytic and historical investigation.

Aristotle looked upon man as "by nature a political animal," and Darwin agreed with him in supposing that man was born a social being. That this is usually true now is certain; to suppose that it was so originally seems gratuitous. It is easy to refer to the fact that man is derived from a characteristically gregarious stock, but the apes nearest man do not live in societies; it is easy to assert that in his primitive weakness man could not have survived in a Robinson Crusoe condition, even with a mate to help him, but we know of many savages who get along fairly well with nothing beyond domestic organisation.

But by some means or other, probably along various paths, man became definitely social, and evolved

* The aim of this chapter is to indicate some of the lines which are now being followed in sociological inquiry.

around himself a social environment. On this he acts, and it reacts on him. This social environment, called in brief a society, is a more or less complex system of inter-relations of thought, feeling, and action, which find expression in traditions and customs, in laws and institutions, in science and literature, in arts and crafts, and so on. Sociology aims at the scientific study of this society—in its present structure and functions, in its origin and development (looking forward as well as backward); and thus, at certain points, it necessarily comes into contact with psychology, anthropology, and history, not to speak of economics (which has primarily to do with industrial organisation) or of politics (which has primarily to do with the affairs of the state as such).

Just as Biology includes Botany and Zoology, Anatomy and Physiology, but is their synthesis rather than their sum, having to do with the fundamental problems of the nature and origin, continuance and progress of living organisms, so sociology, while embracing a number of more special enquiries (which may be separated off if this is found convenient), has to do with the general phenomena of the structure and activity, development and evolution of social groups or of social forms. But just as there has been some disadvantage in separating Biology from the more special disciplines—namely, that many investigators ignore general problems; so there is some disadvantage in defining off Sociology, in so far as it furnishes an excuse for experts—whether historians or economists, anthropologists or psychologists—to pursue their enquiries without recognition of the sociological basis.

To sum up the section, *the justification of sociol-*

ogy as a separate science rests upon the fact, that "man is (now, if not from the first) a social being; his existence is bound up with the community . . . and no individual is complete by himself" (Schäffle). *Every societary form is, in other words, to some degree an organic unity, and more than the sum of its parts.*

HISTORICAL NOTE.

In one sense sociology is old; from Aristotle and Plato to Hobbes and Locke, many had pondered over the problems of society and said wise things about them. But if this be put aside as being not "science," but "philosophy," political or social, then sociology is indeed young and dates from Comte and Spencer.

(1) The term "*Sociologie*" is due to Comte (1839), who had clearly before him the ideal of a study of society which should be dispassionate and free from transcendental assumptions, which should in fact follow the scientific method. His remarkable combination of mathematical and historical attainments enabled him to give an outline of what the work of the sociologist should be—an analytic and historical study of social statics and social dynamics; but he lacked the key which the Evolution-idea affords. Moreover, he meant by the term sociology to include more than is now implied,—he thought of a summation or synthesis of all science with practical reference to the regulation of human society. Comte's *Sociologie* was to *supplant* politics, economics, and much more; but the modern sociologist's dream is rather that of affording the special departments a more secure foundation.

(2) Herbert Spencer, on the other hand, approached the subject as an evolutionist, and although his first book was called *Social Statics* (1850), he consistently regarded man and his social institutions as *products*,—as the results of long processes of change, and as still subject to change. Whether the problem be that of the transition from militarism to industrialism, or the status of women, or the development of law, he showed that the facts were illumined by the light of the evolution-idea. Through the ages man has been adapting himself to the physical environment, becoming more and more its master as he became its more skilled interpreter, and likewise adapting himself to his social environment which is his truest discipline of character. From the antagonism of small groups competing for the means of subsistence to the co-operation of nations in a "*Friedenspiel*," there is a long evolution, but the steps, through pain to further progress, through struggle to greater sociality, are still in part discernible for our guidance; and it is part of the sociologist's task to make them clear.

The central ideas of Spencer's sociological work are thus summed up by Prof. F. H. Giddings,—

"Mr. Spencer's propositions could be arranged in the following order: (1) Society is an organism; (2) in the struggle of social organisms for existence and their consequent differentiation, fear of both the living and the dead arises, and for countless ages is a controlling emotion; (3) dominated by fear, men for ages are habitually engaged in military activities; (4) the transition from militarism to industrialism, made possible by the consolidation of small social groups into large ones, which war accomplishes, to its own ultimate decline,

transforms human nature and social institutions ; and this fact affords the true interpretation of all social progress."

"Such, in its chief theoretical conceptions, is the great sociological system put forth by a master mind, to which all other modern systems of sociological thought, and all more special sociological studies, in one or another way are related." *

(3) As it seems to us, a third historical step of the greatest moment is marked by the work of

"Frederic Le Play, an economist whose name is strange to most people, even to most Frenchmen, but whose thought has none the less been in many ways widely and popularly active throughout the century, and has been and is even now silently working in many channels, at first mainly practical, but now also theoretic and speculative. There are social workers and social students who would estimate his influence on action and his impulse towards thought as alike quite among the very greatest in actual value and in probable usefulness which the nineteenth century is handing towards the twentieth, and this with no disrespect to or forgetfulness of its many great and better-known personalities and forces." †

Le Play turned a fertile brain and a remarkable organising genius to the problem of the concrete interpretation of existing social groups in terms of the three biological categories,—Environment, Function, and Kinship, or, as he phrased it, "*Lieu, Travail, Famille.*" We shall return to this threefold interpretation in a subsequent section.

* *Modern Sociology*, *Internat. Monthly*, Nov., 1900, p. 543. See also his *Principles of Sociology*.

† Prof. Patrick Geddes, *Man and the Environment*, *Internat. Monthly*, I. (1900), p. 179.

LINES OF SOCIOLOGICAL ENQUIRY.

The lines of sociological work are parallel to those in biology:—

(A) Describing the structure of society or of societary forms = Social Statics.	Comparable to Morphology.
(B) Analysing the activities of society or of societary forms = Social Dynamics.	Comparable to Physiology.
(C) Inquiring into the growth of society in whole or in part.	Comparable to Genealogy (Embryology, Palæontology, etc.).
(D) Inquiring into the factors of social evolution (variation, selection, etc.), or into the factors in the evolution of any particular form or function of society.	Comparable to Ætiology, but it need not be separated as a special department as it must be our way of looking at the whole.

It may be of service to illustrate this classification by means of some representative examples. These are indicative of some of the steps of nineteenth-century sociological work, but it should be noted (1) that many of the best pieces of work traverse the whole field, and that even when an investigator refrains from enquiring into the historical or evolutionary aspect, he usually brings some evolutionist ideas into his morphology; (2) that, as before said, the lines separating sociological enquiry from anthropology, psychology, and history (in the narrow sense) are artificial lines of convenience; and (3) that the great bulk of sociological work (we do not refer to sociological *ideas*) is subsequent to Herbert Spencer's finely conceived introduction to the

study * (1873) which was a powerful influence in awakening and diffusing interest in the subject.

A. *Morphological:*

1850. Spencer, *Social Statics.*
1875–8. Schäffle, *Bau und Leben des socialen Körpers.*
1889. Comte de Lestrades, *Elements de Sociologie.*

B. *Physiological:*

1883–1897. Lester Ward, *Dynamic Sociology.*
1893. Emile Durkheim, *De la division du travail sociale.*
1893. Loria, *Les Bases économiques de la constitution sociale.*
Grosse, *Die Anfänge der Kunst.*
Wallaschek, *Primitive Music.*
1896. Lilienfeld, *La pathologie sociale.*

C. *Geneological:*

1861. Sir Henry Maine, *Ancient Law* (Patriarchal Theory).
1861. Bachhofen, *Das Mutterrecht.*
1877. Lewis H. Morgan, *Ancient Society.*
1885. McLennan, *The Patriarchal Theory.*
1888. Starcke, *Die Primitive Familie.*
1891. Westermarck, *The History of Human Marriage.*
1896. Giddings, *Principles of Sociology*, Book III.

D. *Ætiological:*

Buckle, *History of Civilisation.*
J. Stuart Glennie, *Theory of the Conflict of Races, New Philosophy of History.*†
1883. L. Gumplowicz, *Der Rassenkampf.*
1890. Simmel, *Ueber Sociale Differenzierung.*
1893. Novicow, *Les Luttes entre sociétés humaines.*
1893. Lester Ward, *Psychic Factors in Civilisation.*
1893. Ammon, *Die natürliche Auslese beim Menschen.*
1897. Baldwin, *Social and Ethical Interpretations.*

* *The Study of Sociology*, International Science Series, 1873.

† I beg to be allowed as a grateful personal tribute to direct attention to the importance of Mr. Stuart Glennie's work, not only as a sociological investigator and original thinker; but also as an evolutionist whose early theory of the importance of the conflict of races (long previous to that of Gumplowicz and contemporary with Darwin's) has been unjustly lost sight of. He is also one of those who have persistently endeavoured to carry on into the sciences dealing with organisms the laws and lessons of inorganic phenomena.

THE SOCIAL ORGANISM.

The comparison of society to an organism is at least as old as the philosophy of Plato and Aristotle, and the analogy has been a favourite one in many minds. It has been made the keynote of what is often called "biological sociology," it is especially valuable in correcting mechanical ideas; but like many another analogy, it has been overworked.

As Spencer was one of the first to fill in the analogy with biological detail, we may refer to his comparison. In a famous essay in 1860 he compared government to the central nervous system, agriculture and industry to the alimentary tract, transport and exchange to the vascular system of the animal. He also pointed out that, like an organism, a society grows and differentiates, and so on.

While Spencer is largely responsible for the diffusion of the analogy between a society and an organism, it should be carefully noted that it was he who introduced the term "super-organic" as descriptive of society, indicating thereby that the biological conceptions may require considerable modification before they can be safely used in sociology.

It is obvious that the analogy may be pursued far. A society may be compared to an organism as regards the genetic kinship of the component units (the cell = the individual or the family?); in the power of retaining integrity or equilibrium in spite of ceaseless changes both internal and external; in the internal struggle of parts which co-exists with some measure of mutual subordination; in owing its peculiar virtue to the subtle inter-relations between

its elements; in its power of coalescing with another form or of giving birth to another form; in its habit of competing with other forms, as the result of which adaptation or elimination may ensue; and so on. The analogy is far-reaching and persuasive, and it is helped over some of its difficulties by the consideration that just as there are many forms of social-group, from the nomad herd to the French Republic, so there are many forms of organism from sponge to eagle.

Schäffle, in his famous work on the *Structure and Life of the Social Body* (1875), carried the metaphor of the social organism to an extreme which has induced many to recoil from it altogether. The family is the cell, and the body consists of simple connective tissue (expressed in unity of speech, etc.) and of various differentiated tissues, including a sensory and *motor* apparatus, and so on. The comparison is as interesting as a game.

In his lucid exposition of the modern outlook,* Professor Fairbanks admits that a society deserves to be called *organic,* because of its structural complexity; its dynamical unity of correlated parts; its unity and development determined from within (surely not wholly?); its dependence on the environment, both physical and social; and its intelligibility only as part of a larger process,—the evolution of human society as a whole. But he adds that a society differs from a "biological organism," let us say a bird, in the greater original discreteness of its elements, in its less fixed and permanent form, in the greater interdependence of the parts, and in the fact that consciousness remains centered in the discrete individual elements. Perhaps the enthu-

* *Internat. Journ. Ethics*, VIII., 1897, p. 61.

siast for the "social organism" idea would argue each of these points.

There are many other objections to the analogy. Thus Mr. E. Montgomery writes:—"Vital organisation is not brought about like social organisation through the consensus of autonomous units. It is wrought within a unitary being, whose organic differentiations and specifications were gradually elaborated through interaction with the medium. The end of vital organisation is realised in the co-operative efficiency of its constituent parts in total subserviency to the organism as an integral being, whilst the true end of social organisation among us human beings is realised in the social consciousness of each constituent individual." *

But it might be maintained that there is some consensus of units in the making of an animal body, and that in early human societies the consensus was rather enforced than deliberate.

The ideal society is synonymous with humanity, but the reality is far otherwise. For the purposes of scientific study, we must abstract our ideal conceptions, and recognise numerous social groups of men who have, with some bond of unity and with some persistence, come to share a common life. *Such a social group is the unit in sociological study.* It is more than a sum of individuals just as an organism is more than the multitude of its cells, just as a molecule is more than the sum of its atoms; in other words, it has a unity, it is an integrate. The unity might be more assured, the integration might be more perfect, but without some unity or integration there is no social group in the sociological sense. A casual assortment of individuals, isolated for in-

* *Internat. Journal Ethics*, VII., 1897, pp. 414–434.

stance by shipwreck, is not a social group, though it might become one. The Pilgrim Fathers, on the other hand, formed a social group. Until there is enough of unity for the group to act, however imperfectly, *as a group,* contradicting the egoism of the isolated individual, there is no society.

The chief objections to the analogy, as it seems to us, are:—(1) that every societary form we know is an imperfectly unified integrate of organisms, and that the analogy is rather between society and ant-hill or bee-hive or beaver-village than between a society and an animal body; (2) that the unity which the social philosopher looks for is "a unity which is the end of its parts," but though this is clearly distinct from a mechanical unity, it is rather an ideal than a reality either in society or in an individual body; and (3) that since the biologist has not yet been able to discover the secret of the individual organism, notably the secret of its unity, the comparison is suggestive of an attempt to interpret *obscurum per obscurius.*

In thinking of the unity of the individual organism—which seems to us an unsolved problem—we have to distinguish (*a*) *the physical unity* which rests on the fact that all the component units are closely akin, being lineal descendants of the fertilised ovum, and on the fact that they are subtly connected with each other, whether by intercellular bridges or by the commonalty established by the vascular and nervous systems; and (*b*) *the psychical unity,* the *esprit de corps,* which in a manner inconceivable to us makes the whole body one. There are organisms, like sponges, in which the psychical unity cannot be verified.

The same is true in regard to the social organism;

we have to distinguish (*a*) *the physical unity* which rests on hereditary kinship (what Giddings calls "the consciousness of kind") and on similar life-conditions and (*b*) *the psychical unity*, which rests on the unity of psychical life—the "social mind"—developed within the social group and with relations to certain ends. It seems probable that in early days the physical unity was more important than it was later on, when, in some cases of mixed nations, the psychical bond is practically supreme; and we may still distinguish between groups whose unity is determined by genetic and environmental bonds, from others in which the association is also definitely determined to the accomplishment of particular ends.

If, then, we continue to speak of society as a social organism, we must safeguard the analogy by remembering that its character as organism exists in the thoughts, feelings, and activities of the component individuals. The social bond is not one of sympathy and synergy only, for the rational life is intrinsically social. As Green said "social life is to personality what language is to thought."

"LIEU, TRAVAIL, FAMILLE."

Apart from a corroboration of the evolution-formula, the chief service that biology has rendered to sociology is in indicating the three main *factors in interpretation*,—namely, the environment, the function, and the genetic relations of the organism. (1) The living creature exists in the midst of a sphere of influence (soil, temperature, illumination, weather, other unrelated living creatures, and so on)—which constitutes its environment. That this environment has its grip upon the organism, modifying

it, prompting it to vary, eliminating it, is obvious. (2) To this environment, however, the organism reacts, modifying it, utilising it, and in some measure, perhaps, mastering it. In other words, function consists of action and reaction between the organism and the environment. (3) But in the third place, the organism is in genetic continuity with its ancestry, it is the expression of an inheritance, it has kin and it produces more. All biological interpretations must take account of the three facts:—environment, function, and kinship.

As biology came of age, its modes of interpretation were bound to have their influence on other studies; and this influence on sociology has been far more important than the idea of "a social organism." A method is better than a metaphor.

(I.) To interpret a social form we have to take account of locality, climate, fauna, and flora, and so on, in a word, *Lieu;* (II.) of the mode of life, the occupations, the doing and not-doing, in a word, *Travail;* and (III.) of natural inheritance and the facts of kinship, in a word, *Famille.*

(I.) *Environment.*—Although precise facts as to the influence of the environment on the organism are now more abundant for plants and animals than for man, it was apparently in reference to man that the idea first took hold. The theory that man was moulded by his surroundings is much older than Buffon and Erasmus Darwin, Lamarck and Treviranus who insisted, in various ways, on the environmental factor. But just as exact biological facts of environmental influence were scarce before the work of men like Semper, though interpretations in terms of supposed environmental influence were rife, so it must be confessed that most of the human illustra-

tions still remain on the merely interpretative plane. Nor can it readily be otherwise, for experimenting on man can only be done indirectly. It is, however, of much interest to observe how many workers, from many different sides, are now emphasising the environmental—the geographic—factor. There is a renewal of confidence in the aphorism—*Historiæ alter oculus geographia!* "Tell me the geography of a country," Victor Cousin said, "and I will tell you its future."

That the characteristics of a race are in part due to the influence of the physical environment was an idea familiar to Montesquieu and to Humboldt and characteristic of Le Play and of Buckle, and perhaps there is no one who would now think of maintaining a direct negative. But those who admit the reality of the factor are not unanimous as to its power. The question is, *how much* we can legitimately make the environment responsible for. Thus Buckle regarded the environmental factor as of special importance in relation to what he called primary civilisations, while later on the influence of people on people became more momentous. In other words, man has loosened the grip of the environment, and in many cases his emancipation has made him callous.

It is obvious that the configuration of a country may imply concentration, isolation, accessibility; that climate may partly account for sluggishness or industry, for carelessness or forethought; and that many consequences will follow from the resources of the soil, and the nature of the fauna and flora. The influence of the environmental factor is expounded in many books, e.g., Fairbanks' *Outlines of Sociology;* it seems more appropriate to our purpose to borrow from that work a quotation from

Humboldt:—"The final and highest truths of the geographical sciences are included in the statement that the structure of the earth's surface, and the differences of climate dependent upon it, visibly rule the course of development for our race, and have determined the paths for the changes of the seats of culture; so that a glance at the earth's surface permits us to see the course of human history as determined (or, one may say, purposed) from the beginning, in the distribution of land and water, of plains and heights."

In this section, we are dealing with the interpretation of peculiarities in various societary forms. It may be difficult to decide whether a characteristic should be compared to an "environmental modification" (i.e., the direct effect of external influence, producing a change which transcends the limits of elasticity and therefore persists), or to an environmental adaptation resulting more indirectly from the selection of "variations." But in either case it has to be interpreted in relation to the environment. It is hardly necessary to say that this line of interpretation is not restricted to physical features, but applies to the whole character of the societary form. Thus, without pressing the point, we may simply allude to the thesis that morality is closely correlated to the environmental conditions.

To sum up: *The environmental influences in the widest sense cannot be overlooked in social interpretations. They affect both body and mind, both the individual and the group. But it should be noted that they are conditions rather than causes of social evolution.* "Outer nature," Keasbey says tersely, "may determine the form, but cannot account for the fact of society."

(II.) *Function.*—Biology has also brought to sociology the idea that the structural features of an organ are to be interpreted in relation to its function or activity. The various forms of activity—so numerous in a modern complex society—are for the most parts referable to the obvious needs of mankind. Many of them are pre-figured in the pursuits and industries of animals, which include hunting and fishing, even hints of agriculture and shepherding (in ants), securing shelter and protection, and so on. Love and hunger, if we use the words widely, are the fundamental impulses which sway both animal and human life. We recall Goethe's question:—"*Warum treibt sich das Volk so, und schreit?*" and the answer, "*Es will sich ernähren, Kinder zeugen, und sie nähren so gut es vermag.*"

To get food, shelter, and clothing; to replace the feeling of fear (for dead as well as living!) by a sense of security; to satisfy the sexual impulse and the desire for companionship—these are at once primary and fundamental needs, each of which has been the subject of much sociological research. In many a social group they may be, as it were, masked in the garments of culture, but the fundamental needs remain none the less. When they are unrecognisable, it usually means some morbid condition of body or mind.

We can imagine how long ago in palæolithic days, when men were perhaps for the most part vegetarians, the ravaging of the home by some wild beast, led to an organised chase, and how the pursuers, at last circumventing their enemy, satisfied at once rage and hunger with the warm flesh. We can imagine how more adventurous spirits took to hunting for other reasons, how they brought home the young

jackal or the kid, and the domestication of wild animals began. We can imagine how men imitated the wolves by hunting in packs, or the pelicans in driving the fish shorewards to capture. Even monkeys may use a stone as an instrument or co-operate to lift some heavy object, and there seems no difficult riddle in man's going much further. A shelter is desirable, and it often needs combined labour to build it or make it safe. The home got a hearth, and the fire made itself felt as a socialiser. With home and clothing property began. Not only were beasts brought into service, but men unconsciously followed the ants in making slaves of their captured human enemies, and the resulting greater leisure implied time for thought and for art. From simple stimuli long continued the framework of a society was gradually evolved.

From a study of origins, always so misty, the sociologist passes to surer ground when he traces the evolution of tools and weapons, through the stone, the copper, the bronze, the iron ages, and from simple to complex forms; or when he shows how division of labour, implied in the very fact of sex, becomes more and more marked, the tool-maker being specialised from the tool-user, the warrior from the food-provider, the preparer of skins from the hunter, and so on through the whole list, and often with the most circumstantial verification in existing uncivilised social groups.

Or, again, the sociologist may follow another line of investigation, which is perhaps most characteristic of the school of Le Play and well represented in Britain by the teaching of Prof. Patrick Geddes, that of showing the social effects of the particular modes of life,—hunting, shepherding, farming, and so on.

Just as Dr. Arbuthnot Lane and Dr. Havelock Charles tell us of the modifications wrought on the shoemaker's and tailor's body by his habits of work; just as Dr. Arlidge has given us a monograph on the diseases causally connected with the different modern occupations; so the sociologist seeks to trace the far-reaching influences of the different primary modes of food-getting. Thus hunting may be said to imply a roving, unsettled life, a small tribe with perhaps only a rendezvous, and the evolution of independence, bravery, and wariness; shepherding may be said to imply a larger tribe, less individualism, more corporate life, and the evolution of protective organisation and rights of property; agriculture may be said to imply a still larger population, a settled life, a relief from anxiety, a greater opportunity to use slaves, more leisure, and thence perhaps more civilisation. The importance of the different kinds of diet has been often pointed out, but Prof. Patten has more than anyone done justice or more than justice to the sociological import of food. We recall Claude Bernard's remark in regard to nutrition:—"*L'evolution, ce n'est pas que la nutrition, vue au travers du temps,*" and Moleschott's aphorism "*Der Mensch ist was er isst.*"

We need not, however, give further illustration; the general thesis is plain that *physical needs, changing in expression with the natural inheritance of each race, determine the fundamental functions which are adapted to particular environments; and on the economic life thus resulting the structure of a society in greater part depends.*

(III.) *Kinship.*—The third great set of factors to be borne in mind in all sociological interpretation may be summed up in the phrase genetic rela-

tionship. In virtue of natural inheritance the primitive social group or small tribe has a physical unity, which rises into a psychical one. As blood-relations, they have certain characteristics in common, they respond similarly to similar stimuli, the sense of kinship grows. Peculiarities may be fixed by in-breeding, and a consciousness of distinctiveness may become vivid enough to be expressed in word or symbol. A primitive sense of kinship may rise into an *esprit de corps,* and that to a race-ideal and patriotism. It must be remembered that the *natural inheritance* (which includes psychical as well as physical features, and not only obvious characters like shape of nose, lips, and eyes but less definable characters like fertility) must be distinguished from the hardly less important *external heritage* expressed in custom and myth, law and institution. Both are part of the racial entail, but only the former is organically transmitted.

The sociological importance of the family can hardly be over-estimated, and it should be remembered that the researches of Starcke, Westermarck, E. Grosse, H. Cunow, and others, have tended to undermine the old conclusion of McLennan and Lubbock that a lawless promiscuity prevailed in the early stages of social evolution. There seems no good reason to doubt that monogamy was primitive.

While carefully distinguishing the question of validity from that of origin, it is important to consider the evolutionist thesis that morality had and has one of its centres around the hearth and the cradle.

According to Mr. Sutherland, the content of morality arises from parental, conjugal, and social sympathy, and the sentiment of Duty is regarded as a sys-

tematisation or standardising of sympathy. Although this seems to us to avoid the difficulty of accounting for the distinctively ethical quality of "thinking the *ought*," it sets forth admirably the pre-human expressions of sympathy at many different levels.

Prof. F. H. Giddings in more than one book has elaborated the thesis that like-mindedness, i.e., like-responsiveness to given stimuli, with correlated similarity in cerebral structure, is the basis of social organisation. Sympathetic like-mindedness results in impulsive social action; formal like-mindedness is expressed in tradition and in conformity to existing social standards; rational like-mindedness leads to the development of a public opinion which becomes an intelligent guide to progress.

To sum up, *the three categories of interpretation, Environment, Function, and Kinship—Lieu, Travail, Famille—seem sufficient for a descriptive account of societary forms, but must not be regarded in a merely physical way. Each is rich in psychical meaning. The physical and psychical lines of advance are parallel, and the outcome is an integration of persons.*

CLASSIFICATION OF THE GENERAL FACTORS OF SOCIAL EVOLUTION.

Our knowledge of the factors in social evolution is still vague partly because of the intrinsic complexity of the problem, and partly because of our ignorance of the early prehistoric stages. It is unsatisfactory to use the past as the interpretative key to the present, if we have previously invented many of the features of that past. It is unsatisfactory to adopt biological conclusions as if they must hold good in society, and this is the more precarious since some

of the leading biological ideas were originally suggested to biology by students of social phenomena. There is no other basis than that furnished by historical research, helped by the present persistence of simple societary forms which, if not exactly primitive, do to some extent suggest what primitive conditions may have been like.

Beginning of Society. The problem of the origin of the primitive social group is so difficult that we are forced at present to an eclectic position, admitting the value of quite a number of distinct suggestions.

(a) Some, like Rousseau, have pointed to man's genetic filiation to a stock which shows many illustrations of family organisation and gregariousness. His view may be summed up in the words:—Man did not make society, (pre-human) society made man. To this it may be objected that the apes most nearly related to man are not strictly gregarious.

(b) Darwin and others have supposed that primitive man was too weak to stand alone, and that he was forced in self-defence to be social. To this it may be objected that not a few uncivilized races live in small and scattered groups, with no more sociability than the mild and timorous chimpanzees.

(c) Many have emphasised the function of the family in developing sympathetic feelings, which diffused to a wider circle. Thus Prof. Fiske in his *Cosmic Philosophy* has maintained that the transition from animal gregariousness to human sociality was due to the relations of parents to offspring, the prolonged period of helpless infancy being of especial importance. But the difficulty is to account for the diffusion of domesticity, and it is evident that the consciousness of kind, which

Prof. Giddings emphasises, requires material—some association wider than the family—in order that it may develop.

(d) Spencer and others look to "co-operation in war as the chief cause of social integration." But while the importance of this factor is almost unanimously admitted, there is room for doubting whether it was primitive. Many simple peoples are very peaceful.

(e) There seems much force in the thesis ably expounded by Prof. L. M. Keasbey that the social cement is primarily economic. "A local food-supply inevitably causes families to congregate, and the more concentrated and permanent the source of subsistence, the closer and more enduring is the resulting tribal aggregation. Forest hunters and river-fishers are thus naturally tribal economists. Isolation is not economically advantageous under such environmental circumstances, and being brought together in their own interests, such people are led to become at least semi-social." *

In short, the clan with which sociology begins is an economic institution. "Sociality arose in the first place out of the economic necessity of productive co-operation." But the historical evolution of society is obviously too difficult a subject to be discussed in a few paragraphs. We may refer for a fine example of the modern mode of treatment to Prof. Giddings' *Principles of Sociology* (1896) Book III., where he distinguishes a series of stages:—the anthropogenic stage, the metronymic tribe, the patronymic tribe, the military-religious civilisation, and the economico-ethical civilisation.

Factors in Social Evolution. As in biology, it

* *The Institution of Society, Internat. Monthly*, I. (1900), pp. 355–398.

seems useful to distinguish (a) the primary or originative factors which evoke change in the societary form, and (b) the secondary or directive factors which determine the persistence of particular lines of change.

(A) *Originative Factors.* Social variations may have an individual or a social origin. "The individual," Baldwin says, "produces the new variations, the new things in social matter." * "The individual particularises on the basis of the generalisations which society has already effected, and his activity supplies the essential material of all human and social progress." † The genius—who must be interpreted as an individual "transilient" variation—may be powerful enough to bring about a social variation. This is the truth in "the-great-man-theory" of history.

Social variations may also have a *social* origin. Increase of population implies the internal growth of society, and the structural arrangements which were adequate yesterday may be incoherent to-morrow unless there be differentiating and integrating changes. The societary form passes from one state of approximate equilibrium to another.

One may doubt whether the biologist has a right to speak of self-differentiation or self-integration in regard to a plant or animal, but there is no doubt that the terms are often appropriate to what occurs in a societary form, which is conscious of itself and actually changes itself.

Another source of variation, corresponding to the biological amphimixis (or fertilisation) is to be found in the coalescence of two societary forms.

* *Social and Ethical Interpretation.* 1897, p. 455.

† P. 456.

This never occurs as an accretion from without; it always implies some measure of amalgamation and intermixture, in ideas, if not also physically, and the result is variation. Strong societary forms may exterminate weak ones, but they cannot swallow them as Pharaoh's lean kine did, and be unaffected. The incidentally weaker organisation may profoundly change the stronger, and victory may be after all to the vanquished.

An important consideration, which seems to have been overlooked by some writers, is that the question of the inheritance of acquired characters (transmission of modifications) assumes quite a different aspect when we pass from plants and animals or individual men to societary forms. While it remains true that the natural inheritance of the component individuals probably does not include modifications, and that the changes most to be trusted are the slow organic or constitutional variations, it must not be forgotten that the *external heritage* embodied in tradition and custom, in laws written and unwritten, in literature and art, and so on, admits of what is practically the transmission of acquired characters. Thus *social* modifications induced by environment or function have in social evolution a direct significance.

This note on social inheritance suggests a cross reference to Galton's work on filial regression, which shows us, he says, that even a nation moves as a great fraternity.

(B) *Directive Factors.* The essay of Malthus in 1798 contains the first modern recognition of the sociological importance of "the struggle for existence," a phrase which he used. In the hands of Darwin, Wallace, Spencer, Huxley, and Haeckel, the idea acquired sufficient validity to form the basis of a

sociological theory. Independently of Darwin, in 1859 Mr. J. S. Stuart-Glennie laid emphasis on the sociological importance of the conflict of races, a process in which the conquerors were often the conquered, becoming merged in and modified by those whom they had physically subdued.

The same general idea has been more recently worked out in detail by Gumplowicz in his *Rassenkampf* (1883) and *Grundriss der Sociologie* (1885), who, while rejecting biological analogy, has an essentially Darwinian outlook. He emphasises the ceaseless struggle, alike in peace and in war, and the resulting re-adjustments of social groups, the strong becoming barons, captains of industry, or a cultured caste; the weak becoming serfs, wage-earners, or "the uneducated." But the antagonism ends in some mutual re-adjustments; the weaker are rarely eliminated, at least not rapidly; they are subjected by the stronger to new ends; and the structure of society becomes more complex. "The great merit of Gumplowicz's work is that he constructs his sociology out of strictly sociological materials."

The use of the selection-formula in accounting for social evolution has been denounced by many as illegitimate, but, so far as we can judge, the objections mainly refer to the mistake that some biological sociologists have made in supposing that the form of the selective process in mankind might be inferred *a priori* from the form of the selective process in plants and animals. As Prof. D. G. Ritchie says: "Biological conceptions are certainly less inadequate than mathematical, physical, or chemical conceptions in the treatment of the problems of human society; but an uncritical use of them in a more complex ma-

terial means a constant risk of mistaking metaphors for scientific laws. To adapt a phrase of Bacon's, we might say that the conception of evolution which is adequate in the biological sphere, is nevertheless *subtilitati rerum humanarum longe impar,*—"no match for the subtility of human history." *

(a) In looking to biology for hints as to the factors in social evolution, it is necessary to bear in mind the present security of biological conclusions on the problem of evolution (see Chap. XI), and the fact that the biologist has himself often followed the clew suggested by social processes. There is no small risk of a lamentably vicious circle. We would suggest that sociologists should as far as possible focus their attention rather on the animal *social-group* (the herd, the flock, the bee-hive, the ant-hill, the beaver-village, the rookery) than on the individual organism, for in the latter case the analogy is more remote, and therefore more apt to be illusive. It should be evident that there is no strict analogy between struggle in non-social species and the competition of social groups. Among individual men it is, indeed, easy to find analogues of what occurs among animals, e.g., in the struggle with climate or with Bacteria; but in the distinctively social struggle it is a case of one organisation against another organisation, and physical victory over the component individuals may mean victory for the organisation (as expressed in ideas) of the defeated.

Furthermore, in using the selection-formula, we must be careful to bear in mind that the selection in a progressive society is in part conscious, deliberate, and rational. Selection determined by

* *Social Evolution*, Internat. Journal Ethics, vi. (1896), p. 166.

conscious purpose may be called artificial or rational, as opposed to natural selection, but the distinction is apt to disguise the fact that the general formula remains the same. And if the philosopher wishes to show in the end that we can only understand the whole sweep of the evolution-process in the light of the self-conscious personality towards which it has been making, that morality is not only an element in cosmic life but the reality of it, he should not dwell on the supposed contrast between the cosmic and the ethical process.

But it must be clearly recognised that the selective process may be varied in its form, at different times and in different spheres. It is always a sifting, but the nature of the sieve is variable. A struggle for subsistence around the platter may be replaced by an endeavour after well-being; military competition may give place to industrial; a premium may be put on mutual aid just as markedly as on self-assertion. But the cruder forms of struggle are often persistent, both at the margin of industrial society and in international relations. While we see in the course of history a raising of the level of competition—from a war with weapons to a battle of wits, from individualistic to co-operative endeavour, and so on—what Huxley chose to call a checking of the cosmic process by substituting for it the ethical process, we see, on the other hand, that the pressure of destructive competition still falls heavily upon the laggards, and that if it be not allowed so to fall, evil results.

Even those who, like Novicow, seem to accept the old words "strife the parent of all," recognise that the universal conflict has had many forms. Thus Novicow distinguishes the slow and

irrational conflict (of the past, in great part), including massacres, homicides, brigandage, slavery, persecution, etc., from the more rapid and rational conflict (of the future) which is competitive and argumentative. There is a gradual elimination of certain forms of conflict; even in war *all* destructive devices are no longer considered fair. The most difficult of social dilemmas is, that if the cruder forms of struggle be too mercifully relaxed there is apt to be an undue multiplication of the unfit, who, in sterner conditions, would have gone under,—while, on the other hand, a persistence of the lower forms of struggle is apt to be prejudicial to the development of genius and of art, and other flowers of civilisation.

To sum up: even those who agree with Schäffle, for instance, that "all processes of social development are subject to the law of natural selection," or go the length of saying with him that "the law of the survival of the fittest is the only clear formula for a moral order of the world," must in clearness admit that when all is said and done selection is only the knife which prunes the tree; it directs but does not originate the vital impulse, the persistent growth, the new initiative. And, furthermore, while the logical form of the selection theory remains the same, a real and practical difference did ensue when man became conscious, if not master, of his fate, and began, as it were, to swim in the current in which he found himself floating.

Isolation. A general survey of racial evolution discloses two directly opposite processes:—on the one hand, (a) dispersion, expansion, with (it may be) resulting differentiation as isolation became more marked; and, on the other hand, (b) consolidation,

amalgamation, unification, with (it may be) resulting integration as the social relations became more subtly interwoven.

In both processes the factor which biologists call "isolation" may operate; thus the expansion of groups may involve the geographical isolation of some of their offshoots, and the consolidation of groups may mean a restricted range of cross-fertilisation.

Of no little importance, as it seems to us, is some consideration of in-breeding (i. e., pairing within a limited range of relationship) and cross-breeding (i. e., the pairing of members of distinct stocks). Thus Dr. A. Reibmayr has argued that the establishment of a successful tribe or race involves periods of in-breeding, with the effect of "fixing" or engraining constitutional characteristics, and periods of cross-breeding, with the effect of promoting a new crop of variations or initiatives.

While there is—and, at present, must be—great diversity of opinion as to the best means of securing a healthier "social organism," there is practical unanimity as to the end in view, which may be expressed in the words with which Mr. Spencer closes the third volume of his *Principles of Sociology* (1897):—"Long studies . . . have not caused me to recede from the belief expressed nearly fifty years ago: 'The ultimate individual will be one whose private requirements coincide with public ones. He will be that manner of man who, in spontaneously fulfilling his own nature, incidentally performs the functions of a social unit, and yet is only enabled so to fulfil his own nature by all others doing the like.'"

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